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ABSTRACT

The Alternative Youth Employment Strategies (AYES) Project began as one of the demonstration projects funded under the Youth Employment and Demonstration Project Act in 1980. The program, which features three training models, is targeted toward high-risk, disadvantaged youth, especially minority youths from urban areas who had prior involvement with the criminal justice system. The three AYES models called for (1) a work experience program in which students worked together on community service-type jobs in crews under the direct supervision of AYES work site supervisors; (2) full-time vocational or academic training, including counseling, supportive, and job placement services; and (3) a rotating program of work experience and classroom training, with no period extending beyond two weeks. The AYES program provided 1,082 high-risk youths in three cities (Albuquerque, Miami, and New York) with employment training. Despite the many issues and complexities involved in assessing the program's effectiveness, it was possible to conclude that AYES was at least partially successful in its attempts to increase participants' employability and reduce their involvement with the judicial system. The most consistent and reliable finding was that participants were more likely to find employment at some time during the followup period than were members of the control group (51 versus 41 percent) and had higher mean weekly earnings ($63.16 versus $47.67). However, the program's hopes for improving the quality of employment for participants and changing their work attitudes turned out to be somewhat naive. (Appendixes include a detailed description of program implementation in the AYES project and methodological notes. Sixty-three data tables are in the document.) (MN)

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ALTERNATIVE YOUTH EMPLOYMENT STRATEGIES PROJECT:

FINAL REPORT

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CHAPTER I: INTRODUCTION

For the past twenty years the federal government has funded employment training programs. Beginning with the passage in 1962 of the Manpower Development and Training Act (MDTA), and continuing with the Comprehensive Employment and Training Act (CETA) of 1973 (and its subsequent reauthorization), these programs were designed to increase understanding of employment problems and to develop responsive policies. The scope and target groups of these programs changed over the years, but throughout this period there were research, demonstration and evaluation projects whose primary objective was to provide people with skills necessary to obtain and keep jobs.

Over time, however, the focus shifted from skilled but unemployed persons to unskilled disadvantaged youth. It is members of this latter group who form the target population for the Alternative Youth Employment Strategies (AYES) program in particular, and the programs funded by the Youth Employment and Demonstration Projects Act (YEDPA) of 1977 in general.

The MDTA was directed primarily at helping hard-to-employ adults and youths with work experience obtain institutional or on-the-job training. This act was passed initially because of a belief that new technologies would result in a group of skilled workers who might be permanently unemployed unless they were retrained. The implementation of the training program (and the unrelated decline in unemployment) demonstrated that a larger problem was the pool of poorly educated, unskilled workers who make up the hard-to-employ (Ginzberg, 1980).

During the 1960s and early 1970s the number of programs was increased to include the Job Corps, Neighborhood Youth Corps, and many other CETA programs. With the passage in 1973 of CETA, Congress moved toward decentralization and decategorization. Prior to CETA, money was channeled directly from the Department of Labor (DOL) in Washington to local vocational schools within the public school system. After 1973 funding and principal responsibility for service delivery shifted to the approximately 500 CETA prime sponsors. In 1974, in response to the recession, Congress put into place Title VI of CETA, Public Service Employment (PSE). This program provided about 300,000 public service (PSE) jobs for the cyclically unemployed.

Under the Carter Administration, appropriations for manpower programs were increased, and their scope was broadened. In 1976 CETA was amended to target more funds for the structurally unemployed;* in 1977 and 1978 tax incentives were introduced to stimulate total employment and the hiring of the structurally unemployed; in 1978 CETA was reauthorized; and in 1979, the Carter Administration presented proposals for welfare-reform which would provide 400,000 PSE jobs for the principal wage-earner in families receiving AFDC. Finally, and most central to this discussion, in 1977 the Youth Employment and Demonstration Projects Act was passed.

The Youth Employment and Demonstration Projects Act authorized research, evaluation, and demonstration projects to increase understanding of youth employment problems and to develop policies addressing them. Although the manpower programs of the preceeding 15 years had similar goals, Congress was not satisified that they provided a basis for youth policies for the 1980s (U.S. Department of Labor, 1980). Rather than concentrating on improving conventional programs, earlier programs had focussed on trying new approaches and were not implemented in multiple sites. Additionally research, demonstration, and evaluation projects were not coordinated. According to Robert Taggart, the architect of the YEDPA programs, minimal attention was paid to dissemination of results, and there was a lack of attention given to translation of research findings into policy. To improve coordination, balance, dissemination of results, and operationalization of research, demonstration, and evaluation under YEDPA, annual "Knowledge Development Plans" were prepared.

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* An individual may be considered "structurally unemployed" if he/she has been unemployed for a relatively long period and is a member of a low-income family or is a welfare recipient (Ginzberg, 1980).

As part of YEDPA, multiple-site demonstrations were put into place to test conventional program approaches, and replications of successful demonstrations were attempted. According to Taggart there were four basic dimensions of knowledge development: (1) Complete the multi-year projects which were implemented under YEDPA. (2) Establish longer-term follow-up on already funded activities. The Knowledge Develop ent Agenda set forth in 1979 proposed adding to the research data information gathered from Social Security, income tax, and unemployment insurance records. (3) New knowledge development activities were proposed, especially "planned variations," such as varying the target populations for similar kinds of programs. (4) A final Knowledge Development goal was to operationalize ideas and approaches that were found effective and disseminate products of knowledge development activities.

According to Hahn (1979), the primary concern behind the passage of YEDPA was the jobless rate among teenagers, particularly minority and low-income youth. Thus YEDPA amended Title III of CETA by adding three new programs -- Youth Incentive Entitlement Pilot Projects (YIEPP), Youth Community Conservation and Improvement Projects (YCCIP), and Youth Employment and Training Programs (YETP) -- and created a fourth youth program, Young Adult Conservation Corps (YACC) under a new Title VIII. Programs funded under YIEPP were designed to help low-income youth complete high school. This was accomplished by providing 16-19 year olds from poor households with guaranteed jobs if they would attend school. The goal of YCCIP was to provide 16-19 year old unemployed youth with well-supervised employment that would benefit the community while also developing the youths' vocational potential. YACC was operated jointly by the Departments of Labor, Agriculture and the Interior and was designed to provide youth with occupational skills through work on conservation and other projects. Finally, YETP was intended to improve job prospects and career preparation for 14-21 year old low-income youth with severe unemployment problems.

Funded under YETP, participants in Vera's Alternative Youth Employment Strategies (AYES) project were required to be YETP-eligible, and AYES was part of the Knowledge Development framework. Participants in YETP-funded projects came from families who were at or below 85% of the Bureau of Labor Statistics (BLS) lower living standard income level, and those from families with the lowest incomes were given preference. YETP included formula-funded projects which provided a full range of services for in-school and out-of-school youth (Hahn, 1979).

Beginning in 1978 with the funding of some 30 projects, by Fiscal Year 1980 there were approximately 40 national demonstrations in over 200 sites which used YETP discretionary funds. (AYES was one of the projects supported by these discretionary funds.) These projects covered a wide range of target groups and provided a variety of services. Target groups included in-school youth, rural youth, and out-of-school youth. Programs tested the feasibility of a voucher system for allowing disadvantaged youth to choose educational opportunities; explored school-to-work transition services for high school juniors and seniors; tested the impact of various "service mixes" -- e.g., work experience alone versus work experience with supportive services; investigated the effects on motivation, job awareness, and labor market success of mixing low-income youth with higher-income youth; etc.

The 40 projects receiving YETP discretionary funds, administered through the Office of Youth Programs (OYP), included as part of their design the collection of data using instruments that comprised a Standard Assessment System (SAS). The Educational Testing Service (ETS), under contract with DOL, developed the SAS so that findings could be compared across projects. It was designed to provide a uniform data base for a variety of demonstration projects which would measure program effectiveness on such dimensions as work-related attitudes, job attainment, job retention, and job performance. The SAS provided comprehensive information on participant characteristics to track in-program attitude change, to describe services delivered, and to report on outcomes. It was also intended to further assess the usefulness of the instruments; although the SAS consists of instruments that have been demonstrated to be reliable and valid for disadvantaged populations, revisions were expected as a result of this program of data collection. Finally, the SAS was intended to form a foundation for longer-term impact assessment using Social Security, income tax, and unemployment insurance records.

The SAS consists of pre- and post-tests, to be administered to program participants and control group members at program intake and exit, short-term (3- and 8- month) follow-up surveys, and process surveys. The administration of pre- and post-tests provides short-term impact measures; these, in turn, are used to predict post-program job attainment, adjustment, and earnings (collected on the follow-up surveys). The process surveys were intended to be used to evaluate the effects of staffing, service delivery, program duration, and budget on why program effects were or were not observed. In addition, local labor market conditions would be taken into account in interpreting results.

Thus, before interpreting the outcomes of the AYES research and demonstration project, it is important to place it within the Knowledge Development framework. Building on the experiences of the manpower programs of the 1960s and '70s, the Office of Youth Programs of the Department of Labor embarked upon the projects funded by YEDPA. The Knowledge Development Plans provided a framework within which to approach the general question of "what works best for whom." AYES was the 37th project funded out of YETP discretionary monies, and implemented the SAS research instruments.

The target groups for YETP programs included in-school and out-of-school youth, rural and urban youth and a variety of programs. In April 1979 OYP staff approached the Vera Institute of Justice with a request for a proposal. OYP was interested in including as part of their initiative a project targeted at "high risk youth," and Vera had experience running research and demonstration projects concerning the employment experiences of people involved in the criminal justice system. In response to the OYP invitation, Vera submitted a proposal in July 1979 to design and implement the AYES project. Vera identified its target population as "16-21 year old, unemployed, YETP-eligible youth, who are out of school, and who evidence prior involvement with the juvenile or criminal justice systems, or a substantial likelihood for such involvement in the future". Vera was awarded a six-month planning grant beginning October 1, 1979 during which time it was expected to articulate the program and research designs, select sites for program implementation, negotiate contracts with sponsoring agencies at the sites, and begin to hire senior-level program staff. The project actually began operations in August 1980. This report describes the objectives and operations of the AYES project, the research design employed, the participant population, and the outcomes of the program.

CHAPTER II: AYES: THE PROGRAM AND RESEARCH DESIGNS

Vera, as Central Research Agent (CRA) for the AYES project, assumed responsibility for the following tasks: designing both the research and the program operations; receiving the grant funds from DOL and managing all financial functions pertaining to both program and research; providing technical assistance to and overseeing local operators regarding program implementation; collecting and analyzing all research data; preparing and transmitting data tapes for inclusion in OYP's national database; and drafting all required research reports and reacting to DOL's comments regarding them.

This chapter of the report provides a summary description of the program models and target populations as well as the research design employed in evaluating program impact. For a more detailed account of the programs, the reader is referred to Appendix A which describes the implementation of the program models in each site. In addition, Appendix B presents methodological notes which supplement this chapter's description of the research design.

A. THE PROGRAM

1. Program Models and Objectives. The proposal specified three program models which would be replicated in each of the sites selected for program implementation. The general program models were determined by OYP, with Vera articulating the details. The three models were (1) full-time work experience with counseling and placement services; (2) basic education or vocational or prevocational training with counseling services; and (3) a "mixed" model which offered part-time work, part-time training, counseling and placement. These models were not unique to AYES; DOL has used YETP discretionary funds to support "service mix" projects for other target populations. However, Vera was to assess the effectiveness of the models for youth at high risk of criminal justice

involvement and provide DOL with a database that would permit them to study the comparative effects of different program models with different target populations.

While DOL stipulated that these three program models be offered, Vera designed the particular combination of services that formed the AYES program. Model I, work experience, was of special interest to Vera. Typical CETA work experience involved placement of participants in entry-level jobs in non-profit or government agencies, and such placements were provided for many AYES Model I participants. Vera, however, was already operating a work program (Neighborhood Work Project in New York City) which allowed for closer supervision of the workers and provided a different type of experience for the participants. Therefore, the AYES Model I also permitted participants to work together in crews under the direct supervisionof AYES work site supervisors. The type of work done by these crews included renovation, interior and exterior painting, landscaping, and clean-up jobs for community organizations. This provided them with an opportunity to obtain work experience, knowledge of appropriate workplace behavior (e.g., coming to work on time, wearing the proper clothing, etc.), and interpersonal skills (e.g., accepting supervision and interacting with coworkers).

Participants in full-time educational or vocational training (Model II) were placed in a variety of programs. These included classes in Basic Education, preparation for a high school equivalency diploma, English as a Second Language, and vocational courses in word processing, woodworking, welding, etc. The participants, in consultation with their AYES counselors indicated the type of education or training they wanted. If that kind of placement was feasible, it was provided. Some of these classes were located at the AYES site; others were at vocational schools, community colleges, or other CETA programs. The specific courses and range of available programs varied from site to site. (These and other details of Model II implementation are described more fully in Appendix A.)

Model II participants received day-to-day supervision from teachers at the program in which they were placed; they also received counseling, support services, and job placement services from AYES staff.

Model III participants were expected to spend half of their AYES time in work experience and the other half in classroom training. Whenever possible the two components of Model III participation were related; for example, an individual might take a plumbing class in the morning and spend afternoons at a worksite involving plumbing work. In reality, this model proved extremely difficult to implement (see Appendix A for details). It was often impossible to arrange for complementary work and training or to schedule half-day assignments to each. Instead, the program operators attempted to arrange alternating periods of work and training with no period exceeding two weeks. They were reasonably successful in only one of the three sites (Miami). Thus, in all sites, an imbalance between the amount of work and training or education was the rule. In practice, therefore, the precise distinctions between the experience of participants in Model III and that of participants in the other models were not at all clear.

Participants in all three models were exposed to two counseling systems: the Adkins Life Skills Training and the Vocational Interest, Temperament and Aptitude System (VITAS). The Adkins system was used by counselors in group sessions designed to focus on how to find, get, and keep jobs. VITAS was used to determine types of jobs in which the participant was interested and demonstrated aptitude. Counselors were also available to assist participants with short-term medical, family, housing, and personal problems.

Each participant was entitled to 26 weeks of program participation. Model I participants worked 35 hours per week; Model II and III participants spent 30 hours per week in the program. All participants were paid the minimum wage ($3.10 per hour in 1980 and $3.35 per hour in 1981). Model I participants spent more hours per week in the program because they received wages which were subject to federal and local taxes; Model II participants were paid nontaxable stipends for time spent in classroom training (and Model III participants received wages and stipends as appropriate). When designing the AYES program and research, Vera staff believed it was important to the research that take-home pay be approximately equal for all three models, and 35 hours of taxable income was equal to 30 hours of nontaxed income.

Job placement assistance was a part of the program design for participants in all three models. Using information from program counselors and work supervisors, job developers attempted to place participants in unsubsidized employment upon leaving the program. In fact, for reasons detailed in Appendix A, the job development and placement function was not performed effectively in two of the three sites (New York and Albuquerque) until rather late in the program. Therefore, many participants in these two sites left the program without the benefit of serious job placement assistance from program staff.

The program models in all of the sites were implemented in pursuit of the following objectives: to increase the participants' employment and earnings, as well as their ability to secure and retain employment in the future; and to reduce the participants' subsequent involvement with the juvenile and criminal justice systems through improving their vocational experience.

Because of the research dimension of the project, the participant intake process was lengthy and complicated. That process is described summarily in Section B of this chapter dealing with the research design and in detail in each site in Appendix A to this report.

2. The Selection of Program Sites

Since the AYES research design specified that each site would have 450 participants and an approximately equal number of control group members (see discussion below), potential sites were selected from the pool of 56 cities in the United States with populations of 250,000 or more in 1976.* It was believed that cities with populations of this magnitude would be able to provide a sufficient number of youth who would be interested in and eligible for participation in AYES. Because AYES was intended to serve "high risk" youths, the 28 cities from that pool with crime rates** above the median for such municipalities were sent a brief description of the program and research designs and site selection questionnaires; descriptions and questionnaires were also sent to particular service delivery agents which were recommended by the Department of Labor or were known to the Vera Institute of Justice.

The information collected on the site selection questionnaire was used to prepare a list of ten potential sites and delivery agents which Vera submitted for consideration by the Office of Youth Programs. Several criteria influenced Vera's choice of the ten candidate sites. Vera was interested in operating only in municipalities whose juvenile and criminal justice systems maintained their records in such a way that they would be accessible for research purposes. In addition, it was important that the prime sponsor (or other service delivery agent) in the area have experience with youth programs of similar size and scope as AYES. Vera also considered the rate of (youth) unemployment relative to other candidate cities in the same geographic region; the crime rate (as measured by FBI Index Crimes reported to the police in 1978); the number of youth arrested and/or apprehended; and the ethnic distribution of the population. These data were important because Vera was interested in choosing sites which, together, would provide variation in region and ethnic composition, as well as some variation in unemployment and youth unemployment levels. The questionnaire was intended also to identify agencies with demonstrated track records in delivering services, especially employment services, to young people and which enjoyed good relationships with prime sponsors or delivery agents on the one hand, and criminal and juvenile justice agencies on the other. In addition, information was sought to determine whether there was a workable criminal justice information system to facilitate collection of criminal history, arrest and disposition data. Based on the responses received on the questionnaires, Vera identified one or more municipalities in each of four geographic regions (Northeast, South, Midwest, and West).

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* See Statistical Abstracts, 1978, Table No. 24: Cities with 100,000 population or more in 1970. -- Population, 1950-1976, and Area, 1970.

** Uniform Crime Reports, FBI, 1978.

From this list the Office of Youth Programs selected three cities in which to implement AYES -- Albuquerque, New Mexico; Miami, Florida; and New York, New York. These cities provided regional and ethnic variety, especially among Hispanic groups. Albuquerque has a substantial Hispanic minority (34% of the total population), most of whom are Mexican American and come from families who have been long-term residents of New Mexico. Miami also has a large Hispanic population (33% of the total); however, the Hispanics in Miami are predominantly Cuban, and many are recent immigrants. Miami also has a large black population. The population of New York City could provide a mixture of white, Black, and Hispanic (predominantly Puerto Rican) youth. In Albuquerque and Miami, the local CETA prime sponsors were selected to operate the AYES program. In New York, upon the recommendation of Vera and the City's prime sponsor, the Department of Employment, the Court Employment Project (CEP), a non-profit organization with over 10 years of experience in service delivery to criminal justice involved youths, was selected as the program operator.

3. Defining the Target Population. As indicated above, one factor that distinguished AYES from other YEDPA programs was its target population of high risk youth. Aggregate data on unemployment and crime suggest strong correlations between unemployment rates and crime data such as arrests and prison admissions (Brenner, 1975), and that unemployment and non-participation in the labor force are major problems among youth, especially minority youth in urban areas. Furthermore, official criminal justice statistics indicate that these same groups are disproportionately involved with juvenile and criminal justice systems (FBI, 1978). Because few youth employment programs have focused on high risk youth, and because program impacts on criminal justice system contacts have not been evaluated, the AYES project was designed to fill these gaps.

For the purpose of the project, high-risk youth were defined as 16-21 year old, unemployed, YETP-eligible youth who were out of school, and who evidenced prior involvement with the criminal or juvenile justice system, or a substantial likelihood of future involvement. To increase the likelihood that such youths would be heavily represented among program participants, program operators were instructed to accept at least 50% of their participants on referral from criminal or juvenile justice agencies in the locality.*

4. The Structure of Relationship between Vera and Local Program Operators.

To carry out the CRA responsibilities, Vera established a central staff consisting of Program Officers, a Project Research Director and research staff, and clerical support staff. In addition, Vera hired a Research Associate in each of the three sites and assisted that person in hiring part-time assistants and interviewers. Vera Central Research Staff trained the local researchers and monitored the data collection. Local Research Associates were responsible for the random assignment of eligible applicants to the experimental and control groups; ensuring that research instruments were properly administered to all research subjects; developing subject tracking systems and maintaining contact with research subjects; conducting follow-up interviews; collecting juvenile and criminal justice system data; and transmitting data to Vera Central research.

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* A criminal justice referral was defined as a person referred to AYES by a criminal justice agency, or by a social service agency that originally accepted the person on referral from a criminal justice agency. A referral of the first kind is direct, while the latter type is an indirect criminal justice referral. The question of whether or not the person was under the authority of a criminal justice agency at the time of referral was not relevant to his eligibility for AYES, or his being counted as a criminal justice referral.

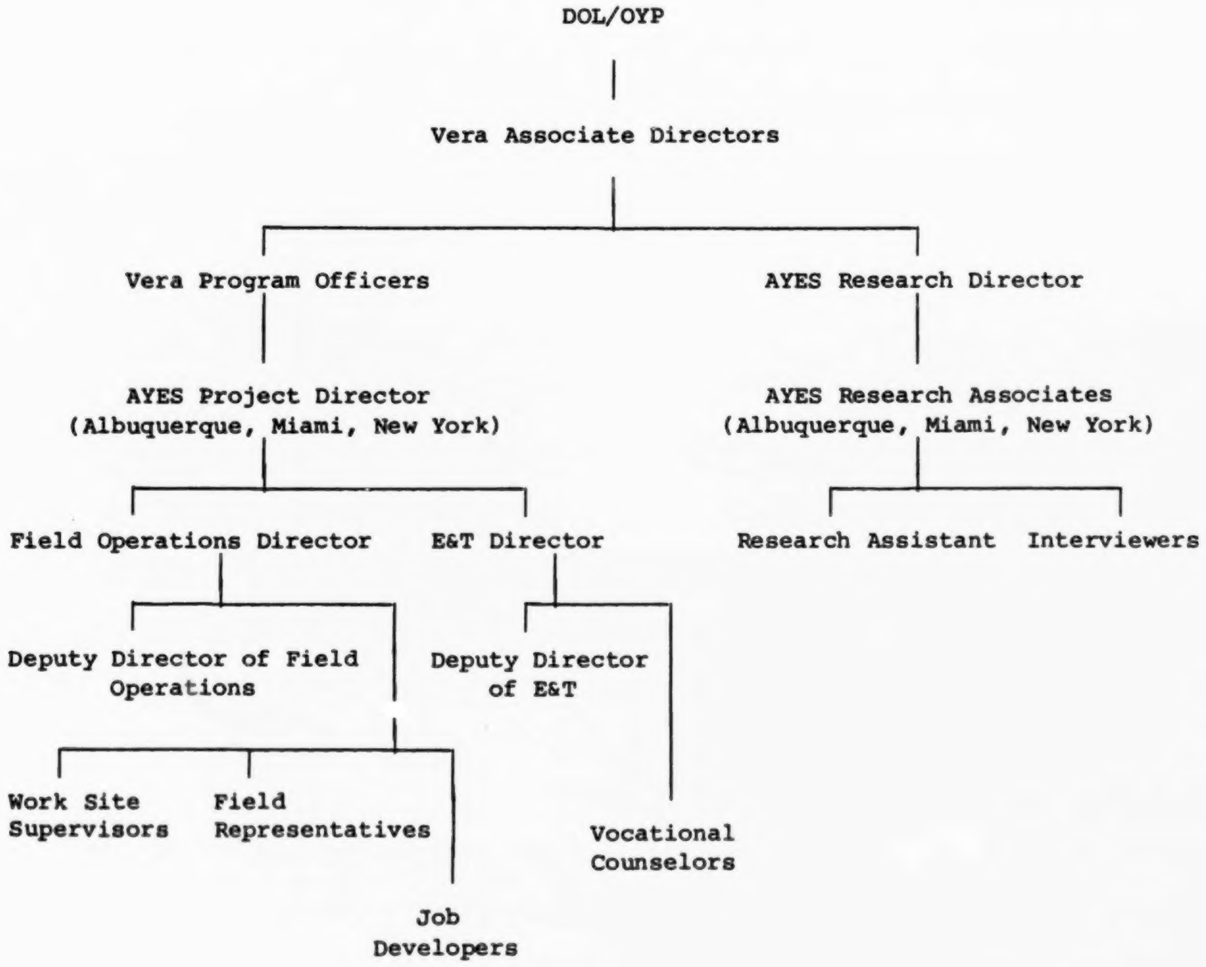
As described above, during the period funded by the Planning Grant (October 1, 1979 - June 30, 1980), Vera compiled a list of cities and service delivery agents which might be potential AYES sites, distributed site selection questionnaires, and submitted its recommendations to the Office of Youth Programs (OYP). Upon the approval by OYP of the three sites and program operators, Vera began program planning and negotiations with local agencies (e.g., potential sources of participant referrals, work site sponsors, and training institutions). During this period Vera further articulated the program and research designs and assisted the local implementing agencies in hiring senior-level program staff.

Once the program had been designed, primary responsibility for operations rested in the sites, and Vera provided technical assistance through its two Program Officers. In Albuquerque the program was operated by the Office of Comprehensive Employment and Training Administration (OCETA); AYES staff were employees of the City of Albuquerque and were hired through the City personnel system. The program was located in an OCETA-owned building. In Miami the program was operated by the South Florida Employment and Training Consortium (SFETC), and AYES staff were employees of the City of Miami. The Miami AYES project was housed in a separate (rented) building in the Liberty City area of Miami. The New York program differed from those in Albuquerque and Miami in that it was operated by a private, non-profit agency, the Court Employment Project (CEP); thus AYES staff were employees of CEP, and the program was located within the CEP offices.

With some variations, the organization of the staff of the three AYES programs was essentially the same. Each site had a Project Director, a Field Operations Director, a Director of Education and Training (E&T), Deputy Directors of E&T and Field Operations, vocational counselors, work site supervisors, and field representatives. The Miami and New York AYES projects also had job developers on their staffs. (For a discussion of job development in Albuquerque, see Appendix A.) The local staffs were hired by the sponsoring agencies, with assistance from Vera Program Officers.

Also present at each site were the local research staff. In Miami and New York, the research staff were employees of the Vera Institute. In Albuquerque, the local research tasks were subcontracted to the Albuquerque Urban Observatory (of the University of New Mexico), but continued to be monitored by the Vera Institute to ensure compliance with the research design and comparability with the other two sites. Having the local researchers employed by Vera rather than by the local AYES projects was a conscious attempt to maintain the integrity of the experimental design. As is described in greater detail below, eligible applicants were randomly assigned to participate in AYES or to be members of a control group. Vera Central staff believed that local researchers who were Vera employees would be less susceptible to any pressures that might compromise the random assignment of individuals to the research groups. Thus, the local research staffs were supported on Vera Central budgets, but housed in AYES offices. Furthermore, while AYES program staff reported to the local Project Directors (who were monitored by Vera Program Officers), local research staff reported to the AYES Research Director at Vera. This somewhat complex structure is described graphically in Figure 1.

FIGURE 1: AYES REPORTING STRUCTURE


DOL/OYP
Vera Associate Directors
Vera Program Officers
AYES Research Director
AYES Project Director (Albuquerque, Miami, New York)
AYES Research Associates (Albuquerque, Miami, New York)
Field Operations Director
E&T Director
Research Assistant
Interviewers
Deputy Director of Field Operations
Deputy Director of E&T
Work Site Supervisors
Field Representatives
Vocational Counselors
Job Developers

B. THE RESEARCH DESIGN

1. The Experimental Nature of the Design

As a demonstration project, an essential part of the AYES program was measuring its impact on the labor market experiences and criminal justice involvement of participating youth and determing whether the impacts varied for the different program models. To determine whether changes observed in participants could be attributed to program, Vera constructed an experimental design. Admission to the program was randomly withheld from approximately half of the eligible applicants. This procedure, which is described further below, provided confidence that the experimental and control groups would not be significantly different from each other at the beginning of the program.

During the project planning period, Vera considered, in addition, randomly assigning participants (experimentals) to particular program models. Once again, it was believed that such a procedure would assure equivalence among the groups at program start and thus provide the logical grounds for attributing differences in outcome to the models. However, after further consideration, the Vera research staff concluded that this second level of randomization would threaten the viability of the program, and, in the end, would not give an assurance of equivalence, after all. Based on Vera's own experience in operating employment projects and serving court referrals, as well as the intake experience of at least one other OYP funded project, the research staff came to believe that random assignment to model would substantially increase the rate at which participants failed to show up or dropped out of the program prematurely. It was reasoned that this would result in fewer people having a meaningful experience with the program. Moreover, since there was no reason to assume that the increased drop out rates would be uniform for the three models, there was no theoretical assurance of group equivalence at the start of the program.

For these reasons, Vera recommended that participants be assigned to model by the "guided choice" method; that is, based on the participants' wishes and the program counselor's assessment of their needs. OYP, however, continued to insist on some use of random assignment. Eventually, a compromise was reached which provided for the first 225 experimentals in each site to be assigned by guided choice and everyone thereafter to be assigned randomly.

As indicated in the chapters that follow, premature drop out was fairly substantial under both assignment procedures and appears to vary by model. Therefore, the analytic strategy used to compare outcomes for different models involves an analysis of the three groups of participants in terms of the differences, if any, that existed among them at intake. Those differences are then used as covariates in regression analyses of various outcome measures.

The basic design was replicated in each of the three sites: each site had as its goal the random assignment of an approximately equal number of eligible individuals to the experimental and control groups. The experimental group consisted of those individuals randomly assigned to participate in the program, and the control group consisted of those eligible individuals for whom participation was randomly denied. The maximum length of program participation for any individual was six months (26 weeks), and the intake period was originally designed to last seven months so that at any one time, there would not be more than 225 active participants per site. (A detailed description of the intake process at each site is presented in Appendix A.) The numbers of experimentals (by program model) and controls who were part of the research sample are presented in Table 1 below.

TABLE 1

Number of Subjects In AYES By Site And Model

	MODEL			CONTROL	TOTAL
Site	I	II	III		
Albuquerque	91	103	101	323	618
Miami	127	113	136	393	769
New York	139	139	133	421	832
TOTAL	357	355	370	1137	2219

The random assignment to the experimental or control group was done by the local Research Associate. When a group of applicants had completed the research intake instruments (see description below), the researcher used a table of random numbers to assign the applicants to research groups. Great care was taken to explain the necessity for random assignment, first to potential referral agencies and second, to those eligible individuals denied AYES services. (For a more complete description of the debriefing of control group members, see Appendix A.)

Once assigned to the experimental group, AYES participants were assigned to program models by either the guided choice or random method. The first 225 experimentals in each site were assigned to one of the three program models (N=75 in each) through a "guided choice" process while the remaining participants (approximately 70 in Albuquerque, 151 in Miami, and 186 in New York) were randomly assigned to models. (The random assignment to model was also done using a table of random numbers.) Under both assignment methods, the site Research Associate did the model assignment. It was the responsibility of the Research Associate to ensure that the models were filled at appproximately the same rate. Under the guided choice procedures, program counselors submitted participants' first and second model choices. The research associate would assign participants to their first choice if there was room in that model; otherwise participants received their second choice. Under random assignment the research associate would determine the number of vacancies in each model, and then randomly assign the appropriate number of participants to each model.

Having two types of model assignment procedures created an additional research variable. While this variable is considered in the analyses described in subsequent chapters, it should be noted that it is confounded with time. That is, participants who were assigned to model through guided choice experienced AYES during its early stages and thus could have been affected by problems related to program start-up. Furthermore, they were released into the labor market during a different period than were those who were randomly assigned to model, and they may have come from different referral sources than did later participants. On the other hand, those participants who were randomly assigned to model came into the program later and may have experienced negative effects of program wind-down, or may have benefitted from the program having had six or seven months of operating experience. It is impossible, therefore, to separate the effects of time from the effects of assignment type on program outcomes.

With site and treatment (experimental or control) as the independent variables of primary interest, the AYES research was designed to measure program impacts on employment and criminal justice system involvement. In addition the Standard Assessment System (SAS) developed by ETS contained instruments to measure participants' (and controls') acquisition of various skills, perceptions, and attitudes that might enhance their employability. Major outcome measures included the ability of participating youth (as compared to that of controls) to acquire work upon program completion; post-program earnings; short-term (up to 8 months) post-program job retention; and the involvement of research subjects with the criminal justice system both during the program and for a short time thereafter.

2. General Hypotheses

A large number of specific hypotheses were tested by the research and these analyses are reported on in the chapters that follow. For the most part, those specific questions and hypotheses were derived from a set of more general ones with which we began the research. These included:

a. Individuals with higher levels of human capital (e.g., formal training and general experience in the labor market) would have better labor market outcomes.

b. The program would add to the participants' level of human capital and, therefore, experimentals would have better labor market outcomes than controls.

c. To the extent that individuals' labor market experience was in the secondary market, there would be no systematic relationship between levels of human capital and labor market outcomes. In this regard, program participation would be considered another secondary market experience. This hypothesis was actually an alternative to that presented as (a) above.

d. Older individuals within the truncated age distribution of the sample (16-21) would evidence better labor market outcomes than younger persons.

e. In-program and post-program contacts with the criminal justice system would be fewer for experimentals than for controls.

Because the literature pertaining to inter-model comparisons was scarce, hypotheses were not specified for them, but provision was made for analyzing the differences among the models for each of the outcome variables. In fact, the analyses of all outcome variables would fall into three categories, as follows: comparisons between experimentals and controls; inter-model comparisons; and analyses of factors outside the experimental design. Thus, the primary analyses would attempt to measure differences between experimentals and controls on variables measuring post-program employment and arrests. The analysis would then shift to identifying differences among the models with respect to these outcome measures. Finally, regardless of differences, or lack thereof, between experimentals and controls or among the models, a third level of analysis would be used to identify those variables that predict employment, earnings, arrests and success in the program.

Data Collection Instruments

Data were collected using standardized instruments developed by the Vera Institute and by the Educatonal Testing Service (ETS) under contract to DOL. Additional data were obtained from official criminal and juvenile justice system records. The instruments used to collect the research data and the processes involved in data collection are described in greater detail below.

Research data were collected from subjects (both experimentals and controls) on four separate occasions over a period of up to fourteen months. The points of data collection were at program intake (prior to assignment to experimental and control groups); at program exit for experimentals and at six months after intake for controls; at three months after exit*; and at eight months after exit. This schedule of data collection was designed by ETS/DOL and was implemented in each of the projects using the SAS, so that the time periods covered by the research data for each of the projects would be comparable. At each of these points, data were collected on instruments developed by the Vera Institute and on instruments developed by ETS for the Department of Labor.

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* Throughout this report, "Exit" refers to the arbitrary date six months after intake for controls and to the actual program termination date for experimentals.

The instruments designed by ETS were intended to measure a series of work-related attitudes and indicators of job knowledge, the program staff members' evaluations of participants behavior while in the program, and the post-program employment experiences of research subjects. The instruments administered at intake were a reading comprehension test (STEP), an Individual Participant Profile (IPP), and a pre-test battery. The pre-test battery consisted of seven scales: vocational attitudes, job knowledge, job-holding skills, work-related attitudes, job seeking skills, sex stereotypes of adult occupations, and self-esteem. The IPP contained demographic descriptions of the research subjects, including control group members. The SAS materials administered at exit were a second part of the IPP (for experimentals only) which sought information about program participation; the post-test battery, which was identical in form and content to the pre-test and was administered to both experimentals and controls; and a Program Completion Survey for experimentals and a Control Group Status Survey for controls. These latter two instruments were individually administered interviews containing questions about education, training, and employment experiences since intake. In addition, the program's work site supervisors and counselors completed evaluations of the participants' performance while in the program.

Thus, the SAS data collected at exit was intended to provide short-term program impact measures; that is, changes over time on the pre- and post-test measures could be compared for experimentals and controls. Furthermore, using data collected in the individual interviews, comparisons could be made between experimentals and controls on employment status at exit, and educational and training achievements experienced between program entry and exit. The SAS also contained follow-up surveys to be administered individually to both experimentals and controls at three and eight months after exit. These interviews contained questions regarding the subjects' post-program employment, school, training, and social experiences. These data would provide longer-term outcome measures and could be used to compare experimentals and controls; analyses on these outcomes could also be computed using gains on the pre/post-test battery as covariates.

At each of the four points of SAS data collection, a Vera interview was also administered to both experimentals and controls. In the Vera intake interview, data for the year prior to program intake were collected on employment, periods of unemployment, education, training, social/marital status, and self-reported illegal activities. An additional section contained items tapping characteristics of the respondents' family life between the ages of ten and sixteen. The exit interview for controls contained questions about employment, unemployment, school, and training during the six months between intake and exit. Experimentals were not asked those questions, but were asked to evaluate their experiences in the AYES program. Both experimentals and controls were interviewed three and eight months after exit. These interviews covered the period since exit and contained questions about working and non-working periods, school, and training activities. Thus for those people on whom intake, exit, and 8-month* follow-up interviews were conducted, Vera had 12 months of pre-intake and up to 14 months of post-intake data on employment, school, and training activities; demographic characteristics (collected on the IPP); AYES staff evaluations of participants; and participants' evaluations of AYES.

Finally, official record data were collected from criminal and juvenile justice agencies. Data on all AYES experimentals and controls were collected for the two years prior to AYES intake and the fourteen months subsequent to intake. The coding forms were developed by Vera staff to provide comparable information from three different jurisdictions. These forms were used to collect information used to analyze program impacts on criminal justice involvement. Criminal history data (prior to AYES intake) were collected for descriptive purposes and to test whether AYES had differential impacts for those participants with prior criminal histories when compared to those who had no prior records. Data were collected from both juvenile and criminal (adult) records, depending upon the individual's age and jurisdiction.

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* While follow-ups were conducted at three-months after exit, this period was also covered in the eight-month follow-up. Furthermore the success rate for interview completion was higher on the eight-month than on the three-month follow-up. Therefore, all reported follow-up analyses were computed on eight-month follow-up data.

4. Data Collection During Program Operation

AYES program (and research) intake began on July 21, 1980 in Miami, on August 4 in New York, and on August 11 in Albuquerque. (The intake process is described in detail for each site in Appendix A.) In each site on each intake day, referral agencies sent applicants to AYES. Those who were certified eligible were sent first to a program official for an orientation to AYES and then to the AYES research staff. The orientation focused on the program, random assignment, the need for continued contact with the research staff, and the confidential nature of the research interviews. The ETS pre-test and STEP test were administered to groups of applicants. When that battery was completed, individual Vera intake interviews were conducted with the applicants. When the group of applicants had completed the research intake instruments, the local Research Associate randomly assigned half of them to the experimental group and the other half to the control group. As was discussed above, prior to the start of intake, referral agencies had been informed that each eligible applicant had a 50% chance of getting into AYES. In addition, the applicants were informed of this probability by the referral agency and by AYES staff during the orientation session. Those persons assigned to the control group received an additional explanation of the random nature of the assignment and were paid a $10 stipend for their time.

In general, the random assignment process worked very well. There were occasional misunderstandings on the part of AYES applicants who were assigned to the control group, referring agencies, and AYES program staff. Nonetheless, the data presented in Chapter III indicate that the random assignment procedure was successful in all three sites, and analysis of demographic data indicates that the experimental and control groups were equivalent at intake.

Once applicants had been assigned to the experimental group, they were sent to a program staff member for further intake processing. As mentioned above, during Phase I of intake this included a guided choice interview. The result of this session was a recommendation of model assignment from the program counselor to the Research Associate. On Friday of each week the Research Associate made model assignments, giving the participants their first choice if possible, but monitoring to ensure the models filled evenly. During Phase II of intake, similar procedures were followed, however, the "guided choice" interview was used to determine placement within model. During this phase the researcher randomly assigned participants to models (as slots were available) using a random number table. Both procedures (guided choice and random assignment) worked smoothly; it had been explained to program staff that, once a model assignment had been made and program participation had begun, changing it would contaminate the research data.* Once experimentals had been assigned to model and completed the rest of program intake, they were given a date on which they would begin program participation.

The second point of research data collection was at program termination for experimentals and six months after intake for controls (hereafter referred to as "exit"). It was the responsibility of the site Research Associate to maintain contact with control group members during this period. As part of that effort, reminder letters were sent to controls three months after intake. These letters contained business reply cards on which the respondent could indicate any changes in address or phone number.

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* Very few requests for model changes were submitted. When a request was made, it had to be approved by both the Project Director and the AYES Research Director, and only in extreme cases were approvals granted.

Experimentals received their exit interviews at program termination. As described in Appendix A, procedures were established for program staff to notify research staff when a participant had completed his/her 26 weeks in the program,or was being terminated from the program for some other reason. Control group members received a $10 stipend for the time involved in completing the post-test, Control Group Status Survey, and Vera exit interview. For experimentals, however, the time involved in completing the post-test, Program Completion Survey, and Vera exit interview was considered part of their program participation, so they were not paid a stipend.

Vera site research staff continued to follow-up both experimentals and controls for eight months subsequent to exit, regardless of whether they had received an exit interview. During this period research staff attempted to locate subjects for a 3-month and an 8-month follow-up; each of these follow-ups covered activities since exit. The three- and eight-month follow-up instruments included a Control Group Follow-up Survey* or Program Follow-up Survey* (for experimentals) designed by ETS and a Vera Follow-up Survey. The ETS three- and eight-month follow-up surveys were identical; the Vera instruments varied slightly. On both Vera and ETS surveys, the areas of activity that were covered included school and training activities, employment experiences, periods of unemployment, and changes in family/social life (e.g., marital status). Both experimentals and controls were paid $10 stipends for each follow-up interview completed.

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* These instruments also contained Employer Rating forms, submitted with the permission of subjects to their employers. The rate of return on these forms was so low, however, that they could not be included in the analyses.

Although site researchers attempted to track both experimentals and controls for the period from AYES intake through eight months after exit, there was some sample attrition. As is inevitable in a longitudinal study, some sample members were unavailable for exit and/or follow-up interviews. Reasons for sample loss included: the subject's moving out of the area; being incarcerated; having provided incomplete or incorrect contact information at intake; refusing to cooperate with the research; and in a few cases, death. Despite the problems encountered in maintaining contact with these subjects, the completion rates on exit and follow-up interviews, as presented in Table 2, below, were relatively high. As indicated above (and discussed in Chapter III), analyses of the complete sample showed the experimental and control groups to be equivalent at intake; however, due to sample loss it was necessary to determine whether the 69% of the experimentals and 58% of the controls who were interviewed at the 8-month follow-up were also equivalent at intake. The results of this analysis are

TABLE 2

Interview Completion Rates By Site And Treatment

	Experimentals				Controls			
	Alb.	Miami	N.Y.	Total	Alb.	Miami	N.Y.	Total
Intake (N)	295	376	411	1082	323	393	421	1137
Exit								
N Completed	269	316	322	907	168	226	228	622
% Completed	90%	84%	79%	84%	52%	57%	56%	55%
3-Month								
N Completed	226	224	225	676	171	192	210	573
% Completed	76%	59%	55%	62%	52%	49%	51%	51%
8-Month								
N Completed	222	287	238	747	194	250	213	657
% Completed	74%	76%	58%	69%	60%	63%	53%	58%

presented in Appendix B, and they lead to the conclusion that, despite sample loss, the experimentals and controls who remained in the sample were equivalent at intake.

In addition to the interview data, researchers in all three sites collected data on all experimentals and controls from official criminal and juvenile justice records. In each site arrangements for research data collection were made with the agencies responsible for collecting and maintaining official records of juvenile and adult arrests and convictions. These agencies included the Police Departments, Family Courts, and Criminal Courts. The researchers collected criminal history data on each subject for the two years prior to AYES intake and for the 14 months subsequent to intake. Because some percentage of the research sample in each site were juveniles for part of the period, it was necessary to collect arrest information on them from the juvenile justice system. All data, regardless of source, were collected on a standard coding form. The post-intake data provided outcome measures to test program impacts on involvement with the criminal justice system, and the pre-intake data provided both descriptions of the prior involvement of AYES subjects with the criminal and juvenile justice systems and data to be used as covariates in analysis of program impacts. The nature and results of these analyses are described in greater detail in Chapter VI.

Although both the ETS and the Vera instruments contained measures of employment, school, and training experiences prior to and subsequent to AYES, the analyses focus on data collected in the Vera instruments. This was done because the Vera data are inclusive of the information collected in the ETS instruments, but are more detailed and more complete. Thus, by using the Vera data, it was possible to compute more sophisticated and complete analyses than would have been possible with the ETS data. Furthermore, the Vera interviews contain data that are more directly comparable with data collected by the Bureau of Labor Statistics and data collected by other Vera research projects. On the other hand, the pre- and post-tests were unique to the SAS, and these data were analyzed in accordance with the specifications provided by ETS. The results of those analyses are reported in Chapter IV of this report.

The Chapters that follow present the results of analyses of the AYES research data. Chapter III is a description of the AYES participants (and controls) at the time they applied to the AYES program. The results of the analysis of the ETS pre- and post-tests are presented in Chapter IV; these comparisons of the experimentals and controls represent short-term outcomes of the program. A discussion of program impacts on employment variables appears in Chapter V; predictions of current labor market theories are tested and the results are presented. Results of analyses on criminal justice outcomes are presented in Chapter VI, including relationships between employment and crime variables. Chapter VII focuses on predictors of program success; these analyses go beyond the experimental design and examine questions of the effects of family background, education, and extent of program participation on success in AYES, as measured by program completion, number of hours of program participation, and post-program employment.

CHAPTER III: THE AYES PARTICIPANTS

In this chapter the AYES participants are described in terms of their demographic characteristics, family background, employment experiences prior to enrollment in AYES, and criminal and juvenile justice histories for the two years prior to AYES intake. Because participants from the three AYES sites differ substantially, all data are presented by site.

In addition, these data were analyzed to determine whether the subjects assigned to the experimental and control groups were equivalent at intake. Because the results of the analyses did not indicate consistent differences between the two groups, most of the discussion below refers to the combined experimental and control groups.*

The intake process itself had some effects on the participant population. Although the project goal was to attract 50-70% criminal justice (CJ) referrals, difficulties in attracting this population eventuated in a 46% CJ referral rate across the three sites. New York was the only site with a majority of CJ referrals (56%); Miami attracted 49% CJ referrals and Albuquerque only 29%. As is described in Appendix A, the slowness of the intake process eventually led to the decision to abandon the goal of 50% Criminal Justice referrals for Albuquerque. The Albuquerque project was better able to attract non-CJ referrals; therefore, this decision allowed them to take in participants more quickly. It was also expected that approximately half of the program participants would be randomly assigned to model, but the unexpected length of the intake process led to almost

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* In New York there were significant differences between the experimental and control groups with respect to ethnicity and gender, specifically, both Hispanics and females were over-represented in the experimental group. However, these differences, though statistically significant, were not strong enough to have any impact on the analyses conducted.

63% of all experimentals being assigned to model through the guided choice process. In New York and Miami, slightly over half of the experimentals received guided choice model assignments; in Albuquerque, where intake was particularly slow, approximately 80% of the program participants were assigned to model through guided choice.

These two inter-site differences also affected the analysis of effects of the program on employment and crime outcomes. Since the percent of CJ referrals should be negatively related to labor force measures and positively related to criminal justice involvement, inter-site variation on the percent of the sample referred by criminal justice agencies should impact most negatively on New York outcome measures and most positively on the impacts of the Albuquerque AYES program. Because the percent of the sample randomly assigned to model was predicted by Vera and site program personnel to reduce the effects of treatment, the disproportionate number of participants assigned to model through guided choice may enhance the relative effects of treatment in Albuquerque.

Demographics of the AYES Population.

The overall mean age at intake was 18.8, virtually the same at each site. AYES attracted a largely male population -- 66%. New York had the highest percentage of males (74%); the Miami sample was 65% male and Albuquerque was 59% male. Less than one quarter of the AYES sample had a high school diploma or a GED at the time of intake. Albuquerque contained the highest percentage of persons with diplomas (43%), New York the lowest (11%) with 16% in Miami. A summary of the demographic characteristics of the AYES sample is presented in Table 3.

Referral type is related to both gender and education (see Table 4). One-third of the non-CJ referrals had diplomas, as compared to only 12.9% of the CJ referrals; nevertheless, the relatively high proportion of Albuquerque AYES

TABLE 3

Demographic Characteristics Of The AYES Sample By Site And Treatment

	ALBUQUERQUE			MIAMI			NEW YORK			ALL SITES		
	E n=295	C n=321	TOTAL n=616	E n=372	C n=392	TOTAL n=764	E n=407	C n=415	TOTAL n=822	E n=1074	C n=1128	TOTAL n=2204
Referral												
Non-CJ (%)	68.6	73.4	71.1	53.7	48.6	51.1	43.7	45.4	44.5	53.9	54.4	54.2
CJ (%)	31.4	26.6	28.9	46.3	51.4	48.9	56.6	54.6	55.5	46.1	45.6	45.8
Assignment												
Random (%)	20.3	*	*	41.3	*	*	45.7	*	*	37.5	*	*
Choice (%)	79.7	*	*	57.7	*	*	54.7	*	*	62.5	*	*
Mean Age at Intake	18.8	18.8	18.8	18.7	18.6	18.7	19.0	19.1	19.0	18.8	18.8	18.8
Gender												
Male (%)	59.5	59.1	59.3	66.6	63.4	65.0	69.5	77.6	73.5	65.6	67.5	66.6
Female (%)	40.5	40.9	40.7	33.4	36.6	35.0	30.5	22.4	26.5	34.4	32.5	33.4
Had a Diploma At Intake												
Yes (%)	44.3	42.0	43.1	17.0	14.5	15.7	8.5	13.7	11.1	22.5	23.6	23.6
No (%)	55.7	58.0	56.9	83.0	85.5	84.3	91.5	86.3	88.9	76.5	76.4	76.4
Race/Ethnicity												
White (%)	10.8	14.8	12.9	2.9	1.3	2.1	2.7	1.5	2.1	5.0	5.1	5.1
Black (%)	7.1	3.7	5.3	74.9	75.5	75.2	69.1	82.2	75.7	53.4	56.4	54.9
Hispanic (%)	78.7	79.7	79.2	22.2	23.2	22.7	28.2	16.3	22.2	40.7	38.0	39.3
Indian (%)	3.4	1.5	2.4	0	0	0	0	0	0	0.9	0.4	0.7
Asian (%)	0	.3	.2	0	0	0	0	0	0	0	0.1	0.1

* Does not apply to controls.

subjects with diplomas cannot be explained by controlling for percent CJ referral. At all three sites the CJ referrals were predominantly male -- over 82%. Over 53% of the non-CJ referrals were male, with New York slightly higher (59%). The relationships among site, referral source, education, and gender -- as well as their effects on outcome measures -- are presented and discussed in Chapters IV through VII. It is sufficient here to note that variation in gender, education, and referral source confounds simple inter-site comparisons on outcome measures.

TABLE 4

Percent With High School Diploma By Referral Source; Sex By Referral Source

	Albuquerque		Miami		New York		Total	
	CJ	non-CJ	CJ	non-CJ	CJ	non-CJ	CJ	non-CJ
	N=178	N=439	N=375	N=392	N=370	N=463	N=1017	N=1201
Education								
Diploma	23.3	52.4	9.6	28.5	8.5	14.1	12.9	33.3
No Diploma	76.7	47.6	90.4	71.5	91.5	85.9	87.1	66.7
Gender								
Male	80.9	50.3	79.5	51.0	85.1	58.9	82.3	53.2
Female	19.1	49.7	20.5	49.0	14.9	41.1	17.7	46.8

Analyses of the independent effects of ethnicity/race and site are complicated by the specific ethnic compositions of each site (see Table 3). In Albuquerque, the vast majority of the sample was Hispanic (79%) -- overwhelmingly of Mexican descent. Most of the remainder were whites (13%). In Miami and New York, the samples were mostly Black and Hispanic. In the former, about 75% were Blacks -- but this included a number of Haitians who had recently immigrated to the United States. Of the remaining 25%, most were Hispanic (largely Cuban). In New York, about 76% of the sample were Blacks, and most of the rest Hispanics (largely Puerto Rican). The entire sample was 5% white, 55% Black, 39% Hispanic, and less than 1% Native American.*

Tables 5A and B present information on respondents' and their parent(s)' birthplaces. In all three sites, the majority of the sample members were born in the same metropolitan area in which they resided at intake. In Albuquerque, over 80% were born in New Mexico and only 2% outside the United States. About half of the Miami sample were born in Miami; other locations included: states other than

TABLE 5A

Respondent's Birthplace By Site; And Mean Age At Arrival In United States

	Site			
Birthplace	Alb.	Miami	New York	TOTAL
Metropolitan Area	64.5	50.7	76.6	64.2
Same State	16.9	4.3	1.0	6.6
Other U.S.	16.7	13.6	10.5	13.3
Puerto Rico	0.2	2.4	5.1	2.8
Mexico	0.5	0	0	0.1
Cuba	0.5	10.7	0.1	3.9
Other Country	0.8	18.3	6.7	9.1
TOTAL	100%	100%	100%	100%
N	616	764	822	2208
Mean Age of Arrival to United States	11.9	15.0	9.1	13.5
	(N=12)	(N=240)	(N=98)	(N=350)

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* This uneven ethnic distribution proved to be a substantial analytic problem. It would be desirable in research on high risk youth to determine whether there were ethnic differences on outcomes, either across sites or in one or another site. Because of the predominance of Hispanics in the Albuquerque program and the virtual absence of whites in the Miami and New York programs, it was not possible to make these distinctions in the AYES data. For example, it is impossible to distinguish an "Hispanic" effect from an "Albuquerque" effect. Therefore, while there are some references to ethnicity in the remainder of the report, no attempt is made to isolate the independent effects of ethnicity.

Florida (11%), Cuba (11%), and other Latin-Caribbean countries (18%). Miami also contained the most recent arrivals to the United States; the mean age of arrivals to the United States in Miami was 15. About 77% of the New York sample were born in the New York City metropolitan area, 11% in states other than New York and 12% outside the continental United States (especially Puerto Rico and other Latin or Caribbean nations).

The data presented in Table 5B indicate that the Albuquerque sample was predominantly at least second-generation New Mexican (over 70% of their parents were born in New Mexico); fewer than 5% of their parents were born outside the United States. In Miami, approximately 22% of the fathers and almost 26% of the mothers were born in Florida, while 40% of the fathers and 39% of the mothers were born outside the continental United States. In New York, 23% of the fathers and

TABLE 5B

Parent's Birthplace By Site

	Father's Birthplace (percent)				Mother's Birthplace (percent)			
	Site				Site			
	Alb.	Miami	N.Y.	Total	Alb.	Miami	N.Y.	Total
Birthplace	(N=549)	(N=693	(N=672)	(N=1917)	(N=592)	(N=742)	(N=770)	(N=2104
Met. Area	33.0	14.1	22.0	22.3	36.3	15.9	22.5	24.0
Same State	39.2	8.2	0.3	14.3	38.3	10.0	0.4	14.4
Other U.S.	22.9	37.1	42.6	34.9	21.3	37.3	46.5	36.2
Puerto Rico	0.2	5.6	23.1	10.2	0.2	5.1	21.4	9.7
Mexico	2.7	0	0.3	0.9	2.4	0	0.1	0.7
Cuba	0.6	13.3	0.5	5.1	0.5	11.9	0.1	4.4
Other Country	1.5	21.7	11.3	12.2	1.0	19.8	9.0	10.6

21% of the mothers were born in Puerto Rico; another 22% of the parents were born in the New York City metropolitan area; and over 40% of both the mothers and the fathers were born in other states of the U.S.

In summary, Albuquerque was the most geographically stable site in the AYES population. The vast majority of those respondents, as well as their parents, were born in New Mexico. Most of the New York respondents were born in New York City, and most of their parents were born in the United States (but usually outside New York State). The Miami sample was by far the most geographically mobile group. Of the three sites, Miami had the lowest percentage of respondents born in the same metropolitan area; the most foreign-born respondents; the highest mean age of arrival to the United States; the highest percentage of foreign-born parents; and the highest percentage of parents who had migrated within the United States.

Family Composition

In the Vera intake interview, respondents were asked, "...whom did you live with for most of the time when you were between 10-16?"* For analytic purposes, the responses were coded into four categories of family composition (see Tables 6 and 7): intact two-parent families; one-parent families (usually female-headed households); reconstituted families (one biological parent and one step-parent or surrogate); other (friends, relatives, foster homes, institutions).

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* The rationale for the selection of a 10-16 year old time frame was determined by the need for reliability and the composition of the AYES sample. A fixed time period is essential for reliability since it would be confusing for both interviewers and respondents if family items addressed indistinct or variable time frames. The lower limit of ten was selected because in survey research the recollection of events prior to the age of ten is often unreliable; the upper limit of sixteen was based upon the 16-21 year old age range of the AYES sample. In addition, prior research conducted at Vera (on the Neighborhood Work Project and on the Court Employment Project) have utilized the same time frame. Finally, several past studies have discovered significant linkages between family life and crime, focusing on family life during early to mid-adolesence (Johnstone, 1976; Strasburg, 1978; Straus, 1979; Wadsworth, 1979).

TABLE 6A

Family Composition By Site

Family Composition	Albuquerque (N=620)	Miami (N=765)	N.Y. (N=822)	TOTAL (n=2208)
1. Intact	49.0%	36.7%	28.3%	37.1
2. One-Parent	39.9	45.2	56.9	48.0
a. mother only	36.9	42.2	54.2	45.1
b. father only	2.9	3.0	2.7	2.9
3. Reconstituted*	2.9	6.0	2.2	3.8
4. Other	8.2	12.0	12.9	11.2
a. foster parents	.7	2.0	1.5	1.4
b. institution	1.1	0.8	2.7	1.6
c. other**	6.3	9.3	8.8	8.2
TOTAL	100%	100%	100%	100%

TABLE 6B

FAMILY COMPOSITION BY REFERRAL SOURCE AND SITE

	Albuquerque		Miami		New York		Total	
Family Composition	CJ N=177	non-CJ N=439	CJ N=374	non-CJ N=393	CJ N=463	non-CJ N=370	CJ N=1017	non-CJ N=1201
Intact	44.1	51.3	34.2	39.2	25.3	31.9	31.9	41.5
One-parent	40.1	39.6	49.5	41.2	61.1	51.4	53.1	43.7
Reconstituted	3.4	2.7	6.7	5.3	3.0	1.4	4.4	3.2
Other	12.4	6.4	9.6	14.3	10.6	15.4	10.6	11.7
TOTAL	100%	100%	100%	100%	100%	100%	100%	100%

* One biological parent and one step-parent or parental surrogate.

** Usually friends of the family and relatives.

TABLE 7

Family Composition By Site And Ethnicity/Race

(in percent)

Site/Race-Ethnicity	Intact	One-Parent	Reconstituted	Other	TOTAL
1. Albuquerque					
White (n=80)	37.5	41.3	10.0	11.3	100%
Black (n=33)	36.4	48.5	0	15.2	100%
Hispanic (n=493)	52.1	39.0	2.0	6.9	100%
Indian (n=15)	40.0	40.0	0	20.0	100%
2. Miami					
White (n=15)	33.3	40.0	0	26.7	100%
Black (n=578)	35.3	47.2	5.7	11.8	100%
Hispanic (n=174)	42.0	39.1	7.5	11.5	100%
3. New York					
White (n=16)	43.8	50.0	0	6.3	100%
Black (n=608)	26.8	57.9	2.0	13.3	100%
Hispanic (n=205)	31.2	54.2	3.4	11.2	100%
4. TOTALS*					
White (n=114)	37.7	43.0	7.0	12.3	100%
Black (n=1220)	31.0	52.5	3.8	12.6	100%
Hispanic (n=870)	45.3	42.5	3.4	8.8	100%
Indian (n=15)	40.0	40.0	0	20.0	100%

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* X^2 = 52.9
df = 12
p = .0001

The modal category of family composition for the entire AYES population was one-parent family (48%); 37% came from intact families. Albuquerque was the only site with a modal category of intact families (49%); in Miami, 37% came from intact families; in New York it was 28%. In the latter two sites, the modal category was one-parent family. Existing literature suggests that there should be positive relationships between family stability and employment variables, and negative relationships between family stability and criminal justice contacts (Bullock, 1973).

There are several factors complicating the relationship between site and family composition: referral source, ethnicity, geographic mobility, and the degree of urbanization at each site. Although a more intensive multivariate analysis of these factors is presented in Chapter VII, some preliminary data are presented below. As indicated in Table 6B, criminal justice referrals were more apt to come from broken homes than non-CJ referrals; this relationship does not affect the relationship between site and family composition.

Race/ethnicity is also related to family composition (see Table 7). Hispanics were most likely to come from intact families (45%); 38% of the whites and 31% of the Blacks came from intact families. Wherever there were enough cases to enable meaningful contrasts to be made, Hispanics demonstrated more stable family structures than either whites or Blacks. Nevertheless, inter-site differences in family composition cannot be explained totally by controlling for ethnicity: Miami Blacks had more stable families than New York Blacks. Among Hispanics, Albuquerque contained the highest percentage of intact families and New York the lowest.

Welfare History

Respondents were also asked whether their families had been on welfare during the time when the respondents were between the ages of 10 and 16. Just under 30% of the total sample indicated that their families had been on welfare all or most of the time, and 59% had been on welfare rarely or never. (See Table 8.) It should be noted that all subjects who lived in institutions, such as orphanages or training schools, were omitted from this analysis.

Welfare history is highly related to both site and family composition. In Albuquerque and Miami, over two-thirds of the respondents stated that their families had rarely or never been on welfare; 20% had been on welfare all or most of the time. In New York, however, 46% of the sample indicated that their family had been on welfare all or most of the time, and 43% rarely or never.

Intact families were less likely to have been on welfare than either one-parent or reconstituted families. Over 75% of the intact families had rarely or never been on welfare; in contrast, the majority of reconstituted or one-parent families had been on welfare at least off and on. Because family composition is related to site, the relationship between welfare history and family composition is presented by site in Table 8. Inter-site differences in welfare history cannot be explained by differences across the three sites in family composition. For example, over 30% of the intact families from New York had been on welfare all or most of the time, in contrast to fewer than 10% of the intact families from the other two sites. In fact, the welfare histories of the New York intact families were as extensive as the welfare histories of one-parent families in either Miami or Albuquerque.

Several possibilities can be offered as potential explanations of this inter-site variation: (1) differences in family economic status; (2) different welfare rules; and (3) different attitudes about applying for welfare. Because virtually all AYES cases came from economically deprived homes, it is unlikely that inter-site differences can be explained by the first factor. Unfortunately, more detailed data on family income level (obtained from program records) are unreliable. While the explanatory power of the latter two factors appears more substantial than the former, AYES data do not provide sufficient information to evaluate their relative merits. In terms of measures of program outcomes, respondents from families with extensive welfare histories can be predicted to have less positive employment outcomes than respondents without extensive welfare histories. Of course, analysis of the relationship between welfare history and

TABLE 8

Incidence Of Welfare Receipt, By Site And Family Composition

Site/Family Composition	All/Most Of The Time	Off/On	Rarely/Never	TOTAL
1. **Albuquerque**				
Intact	6.6	8.2	85.2	100%(N=304)
1-Parent	33.5	18.0	48.6	100%(N=245)
Reconstituted	5.6	38.9	55.6	100%(N=18)
Other	15.9	11.4	72.7	100%(N=44)
TOTAL	18.0	13.3	68.7	100%(N=611)
2. **Miami**				
Intact	8.5	11.0	80.5	100%(N=282)
1-Parent	27.8	15.4	56.8	100%(N=345)
Reconstituted	19.6	4.4	76.1	100%(N=46)
Other	26.3	10.0	63.8	100%(N=80)
TOTAL	19.9	12.5	67.6	100%(N=753)
3. **New York**				
Intact	30.6	12.8	56.6	100%(N=235)
1-Parent	56.5	11.3	32.3	100%(N=471)
Reconstituted	42.1	26.3	31.6	100%(N=19)
Other	27.4	4.8	67.9	100%(N=84)
TOTAL	45.6	11.4	43.0	100%(N=809)
4. **3 Sites**				
Intact	14.1	10.5	75.4	100%(N=821)
1-Parent	41.9	14.1	44.0	100%(N=1061)
Reconstituted	21.7	16.9	61.5	100%(N=83)
Other	24.5	8.2	67.3	100%(N=208)
TOTAL	28.5	12.3	58.9	100%(N=2173)

employment outcomes must control for site, family composition, and parental work histories.

Parental Work History

Parental work histories are presented in Table 9. If a father was present while the respondent was between the ages of 10-16, he usually worked all or most of the time (81%); inter-site variation was minimal. However, since many respondents had no father living with them, only 38% of the AYES population had a father who lived with them and worked all or most of the time. As a result, Albuquerque (with the highest proportion of intact families) had the highest rate of respondents living with a father who worked all or most of the time (47%) and New York the lowest (31%). Most AYES respondents lived with their mothers. Fewer than 40% of the mothers in New York and Albuquerque worked all or most of the time; the modal category for these two sites was mother working rarely or never. In Miami, however, over 50% of the mothers worked all or most of the time. A tentative explanation for the relatively high rates of mother's employment in Miami may be the following: in New York, most single-parents were able to get welfare, which is apparently more difficult to obtain in Miami. In Albuquerque, there were more intact families than Miami, and, therefore, there was less pressing urgency for women to work. (In addition, it may also be possible that the Mexican-American families in Albuquerque were culturally less inclined to permit or encourage mothers to work than the predominantly Black families found in the Miami sample.) It can be hypothesized that working parents facilitate the labor force success of their children by furnishing them with both role models and job networks. The results of a test of this hypothesis are presented in Chapter VII.

Family Arrest History

About 43% of the AYES respondents responded "yes" to the question of whether any member(s) of their immediate family had ever been arrested (see Table 10). Not surprisingly, the rates for CJ referrals were higher than those for non-CJ referrals (49% to 38%). The rate for respondents from intact families (41%) was slightly lower than the rates for respondents from one-parent (45%) or reconstituted (52%) families.

TABLE 9

Parental Work Histories By Site

Parental Work History	Albuquerque	Miami	New York	Total
1. **Mother Worked**				
all/most time	36.8%	50.9%	39.1%	42.6%
off/on	16.4	20.6	11.4	16.0
rarely/never	46.9	28.5	49.4	41.4
TOTAL	100%(N=587)	100%(N=727)	100%(N=769)	100%(N=2083)
2. **Father Worked**				
all/most time	80.8%	78.8%	84.7%	81.2%
off/on	5.6	12.8	5.0	7.8
rarely/never	13.6	9.1	10.3	11.0
TOTAL	100%(N=360)	100%(N=386)	100%(N=301)	100%(N=1047)
3. **Percent with Working Fathers**				
no father	42.2%	49.6%	63.4%	52.8%
worked all/most time	46.7	39.6	31.1	38.4
worked off/on	3.2	6.3	1.8	3.7
worked rarely/never	7.9	4.6	3.7	5.2
TOTAL	100%(N=623)	100%(N=768)	100%(N=835)	100%(N=2226)

The percentage of respondents who reported that a family member had been arrested was lower in New York (37%) than either Albuquerque (49%) or Miami (46%). Since New York had the highest rate of non-intact families and the highest percentage of CJ referrals, family composition and referral source cannot explain this finding. Three hypotheses seem tenable. First, the family size of the New York sample was probably lower than the other two sites, thereby decreasing the probability of having a sibling who had been arrested. (Because the Intake Questionnaire did not elicit information on family size, family size variation is based on Census Data and can yield only ecological correlations; therefore, this hypothesis cannot be tested empirically.) Second, it is possible that the New

York respondents were less open about illegal activities than respondents from the other sites. Significantly lower rates in New York for the Self-Reported Activities (SRA) Scale* (p=.003) lend some credence to this hypothesis. Third, the lower New York rate may be a function of more aggressive police activity in the other two sites. Even though, according to the Uniform Crime Reports (1978), the crime index for major felonies was higher in Miami than either New York or Albuquerque, the police in Miami and Albuquerque may have been more aggressive in making arrests for such crimes as possession of marijuana, truancy, vandalism, and other relatively minor offenses. As a result, the siblings of respondents in these two sites may be more likely to have been arrested than in New York, where such offenses may be more often ignored by the police.

Respondents' Social Situation

The final two family variables addressed the respondents' present social situation and relationship with their children (if any). At the time of the intake interview, 4% of the sample were currently living with their spouses, and an additional 5% were cohabiting. Female respondents were more likely than males to be married. Over 8% of the Albuquerque sample were residing with their spouses, and 7% were cohabiting; the totals for both Miami and New York were considerably lower (see Table 11). Because the mean age at intake was identical in the three sites, age cannot explain this pattern. About 43% of the total sample were seeing a person on a steady basis, females slightly more often than males (46% to 41%). (The relationship between labor force variables and level of interpersonal commitment is evaluated in Chapter VII. Because married and cohabiting respondents are somewhat older than the others, age is controlled in this analysis.)

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* This series of 20 items from the Vera interview elicited information on how many times in the preceding 12 months respondents had committed illegal behaviors.

Nineteen percent of the sample had children, females (30%) more than males (14%). There was no relationship between having children and site. Only 12% of the sample were currently living with their child(ren), however. Not surprisingly, the mean age of respondents with children was slightly but significantly higher than the mean age of those without children. (See Table 12.) There may be a positive relationship in this sample of 16-21 year old males between employment measures and having children (especially those claiming to be supporting those children financially). The relationship between employment and having children among the females is also evaluated, although the nature of that relationship could not be predicted. The relationship between criminal justice involvement and having children is examined in Chapter VII.

TABLE 10

Percent With Member Of Immediate Family Having Been Arrested, By Site, Type Of Referral, And Family Composition

Site,Referral Type, and Family Composition	Yes	No	DK	TOTAL
1. Site:				
Albuquerque	48.5	50.7	0.8	100%(N=621)
Miami	45.6	51.9	2.5	100%(N=765)
New York	36.7	60.4	2.9	100%(N=821)
2. Referral Type:				
CJ	49.1	48.5	2.4	100%(N=1012)
3. Family Composition*				
Intact	40.7	57.8	1.5	100%(N=820)
1-Parent	45.4	52.1	2.6	100%(N=1060)
Reconstituted	51.8	48.2	0	100%(N=83)
Other	36.2	60.2	3.7	100%(N=246)
4. TOTAL	43.1	54.7	2.2	100%(N=2207)

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* X^2 = 18.15 p= .0006 df

TABLE 11

Present Social Situation, By Site And Gender; Mean Age By Present Social Situation

(in percent)

Site/Gender	Living w/Spouse	Cohabiting	Steady	None	TOTAL
1. Albuquerque					
Male	7.6	7.3	35.8	49.3	100%(N=369)
Female	8.8	6.8	38.0	46.4	100%(N=250)
TOTAL	8.1	7.1	36.7	48.1	100%(N=619)
2. Miami					
Male	1.0	3.6	39.3	56.1	100%(N=496)
Female	7.5	3.7	42.9	49.9	100%(N=268)
TOTAL	3.3	3.7	40.6	52.5	100%(N=764)
3. New York					
Male	1.2	6.8	45.6	46.5	100%(N=607)
Female	3.2	2.3	59.4	35.2	100%(N=219)
TOTAL	1.7	5.6	49.3	43.5	100%(N=820)
4. All Sites*					
Male	2.7	5.8	41.1	50.4	100%(N=1473)
Female	6.7	4.3	46.1	42.9	100%(N=737)
TOTAL	4.3	5.3	42.8	47.9	100%(N=2210)
5. Mean Age At Intake	19.4	19.5	18.8	18.8	18.8

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* $X^2 = 29.6$ df = 3 p = .0001

TABLE 12

Percent Living With And/Or Supporting Children By Gender And Site; Mean Age By Parental Role

Gender/ Site	Live w/& Support	Live w/ Only	Support Only	Neither	Total With Children	Total Without Children	TOTAL
Alb.							
Male	6.0	1.1	3.3	1.9	12.3	87.7	100%(N=365)
Female	20.5	4.4	0.4	1.6	26.9	73.1	100%(N=249)
TOTAL	11.9	2.4	2.2	1.8	19.3	81.8	100%(N=614)
Miami							
Male	2.4	0.2	7.7	3.8	14.1	85.9	100%(N=497)
Female	24.2	5.6	2.2	0.7	32.7	67.3	100%(N=269)
TOTAL	10.0	2.1	5.7	2.7	21.5	79.4	100%(N=767)
New York							
Male	2.3	1.0	5.9	5.3	14.5	85.6	100%(N=609)
Female	20.8	5.9	0.9	1.4	29.0	71.0	100%(N=222)
TOTAL	7.2	2.3	4.6	4.2	18.3	81.7	100%(N=831)
All Sites							
Male	3.3	0.7	5.8	4.0	13.9	86.1	100%(N=1477)
Female	21.8	5.3	1.2	1.2	29.5	70.5	100%(N=742)
TOTAL	9.5	2.3	4.3	3.1	19.1	80.9	100%(N=2219)
Mean Age At Intake	19.5	19.5	19.4	19.3	19.4*	18.7*	18.8

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* F = 18.8
df = 4
p = .0001

Pre-Intake Employment History

In the Vera intake interview respondents were asked a series of questions about their prior employment experiences. Included among these were whether the respondent had ever worked; detailed questions about the nature of their most recent job; detailed questions about the nature of the prior job if it occurred in the 12 months preceding AYES intake; questions about periods of non-working during the 12 months prior to intake; and dates of other working and non-working periods during the last 12 months. These data were analyzed to provide a description of the labor force experiences of the AYES subjects and to ensure that those experiences were equivalent at intake for experimentals and controls.

Three-fourths of all AYES subjects had worked at some time prior to intake (this question was not restricted to the preceding 12 months). There were no differences between experimentals and controls on this variable. The percentage of subjects who had ever held a job varied significantly by site,* however, with 83% of the Albuquerque subjects (N=618) reporting that they had worked at some time during their lives, 75% of the New York subjects (N=827) having worked, and 60% of those (N=769) in Miami having worked. While the reasons for this difference are unclear, it is possible that the differences in immigration patterns among the sites are related to the likelihood of having worked. That is, as discussed above, Albuquerque AYES subjects were likely to have been born in New Mexico and have parents who were born in New Mexico. This geographic stability could have aided the Albuquerque respondents in developing networks and familiarity with the local labor market. Similarly, New York AYES subjects were likely to have been born in the New York metropolitan area. Miami AYES, on the other hand, had the largest proportion of foreign-born subjects and the most recent immigrants. Furthermore, of the approximately 60 Haitian AYES subjects in Miami, many indicated that they had never worked. Because the Haitians were located in the Miami site, this ethnic difference may have affected the percentage of subjects in Miami who had ever worked.

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* $X^2=35.81$; $df=2$; $p<.0001$

Other comparisons on having worked at some time included those between Blacks and Hispanics, between males and females, and between criminal justice (CJ) referrals and those from other agencies (non-CJ). Blacks and Hispanics were equally likely to have worked at some time during their lives (whites were excluded from this analysis because there were so few of them in the Miami and New York samples, 2% of each). Males were significantly more likely to have worked (79%) than were females (68%)*. While CJ referrals were significantly more likely than non-CJ referrals to have had a job, this difference is small (78% vs. 73%),** and its statistical significance is likely the result of the large number of cases involved in this analysis (N=2214).

The Year Prior To Intake

Extensive data were collected on AYES subjects' work experience during the year prior to intake. Of these data, the analyses focused on the percent of that year spent employed; dollars earned during the year; average duration (in days) of jobs held during that year; weekly wages for the most recent job; and industry and occupation of the most recent job. For each of these variables, comparisons were made between the experimental and control groups; between males and females; among whites, Blacks and Hispanics; and between criminal justice and non-criminal justice referrals. Each of these analyses was done by site. There were no differences between experimentals and controls in any of the sites on any of the employment variables. There were significant differences on some of the other comparisons, and these are discussed below.

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* $X^2=32$; df=1; $p<.0001$

** $X^2=7.3$; df=1; $p<.0067$

AYES subjects spent a relatively small percentage of the year prior to intake in jobs.* The mean percent of the time employed was 18.4%, varying from 15.6% in New York to 21.3% in Albuquerque. (See Table 13 for details.) Because approximately 28% of the AYES subjects had not worked during the year prior to intake, the percent of the year employed can also be analyzed for only those subjects who had worked during that year. On the average, this subsample had worked for a quarter of the preceding year (varying from 22.0% in New York to 27.6% in Miami).

For the sample as a whole, males had spent significantly more of the year prior to intake employed (19.6%) than had females (15.8%).** However, among those AYES subjects who had worked during that year, there was no difference between the males and females, with both averaging about 25% of the year employed. The analyses of both the total sample and the sample of AYES subjects who had worked indicated that the only statistically distinguishable ethnic group was Blacks in New York; with an average of 15.4% of the previous year working, this group spent significantly less of the year prior to intake in employment than did other

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* Detailed data on employment were collected for jobs of 15 hours or more per week. If the respondent indicated that he/she had worked less than 15 hours per week, he/she was considered not working.

** $F(1,2194)=15.62$; $p<.0001$

TABLE 13

Values At Intake Of Labor Market Indicators For Total Sample And By Site

(Number of Cases in Parentheses)

	Total Sample	Alb.	Miami	New York	Statistics On Hypotheses That Sites Are Equal	
					Statistic	p
Mean % of Pre-Intake Year Employed (All Subjects)	18.4% (2218)	21.3% (618)	19.0% (768)	15.6% (832)	F(2,2194)=9.42	.0001
Mean Percent of Pre-Intake Yr. Employed (Subjects with Employment During Pre-Intake Year)	25.4% (1603)	27.2% (484)	27.6% (528)	22.0% (592)	F(2,1580)=7.94	.0004
Mean Total Earnings During Pre-Intake Yr. (All Subjects)	$879 (2218)	$921 (618)	$986 (768)	$750 (832)	F(2,2194)=5.43	.0044
Mean Total Earnings During Pre-Intake Year (Subjects with Employment During Pre-Intake Year)	$1,383 (1410)	$1,308 (435)	$1,574 (481)	$1,263 (494)	F(2,1386)=4.92	.0074

groups. (See Table 14 for details.) Results for the working subsample are similar, with New York Blacks (21.7%) significantly lower than all other ethnic categories. Nonetheless, because the groups with the highest average percent of time working (Miami subjects and Albuquerque Hispanics) worked less than 28% of the time, this is probably not an important distinction.

TABLE 14

Mean Percent Of Pre-Intake Year Employed

	SITE		
All Subjects	Albuquerque	Miami	New York
White	22.6%	--	--
(N)	(80)		
Black	--	19.2%	15.4%
(N)		(576)	(604)
Hispanic	20.7%	17.8%	16.6%
(N)	(490)	(174)	(205)
Subjects Who Worked			
White	25.4%	--	--
(N)	(71)		
Black	--	27.4%	21.7%
(N)		(403)	(430)
Hispanic	27.0%	27.9%	23.0%
(N)	(376)	(111)	(148)

In the year prior to intake, the average AYES subject earned (from employment) a total of $879. This varied significantly by site from $750 for New York AYES subjects to $986 for those from Miami. (See Table 13) Males, with a mean of $980, fared better than females, whose average earnings were $679.* Among the subpopulation of those who had at least one job during the year prior to intake, total earnings averaged $1383, varying significantly by site, from $1263 in New York to $1574 in Miami. The gender difference was also statistically significant for this subsample: males earned an average of $1464 and females $1193.*

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* $F(1,2194)=23.96$; $p<.0001$, controlling for site.

Given that the average AYES subject had worked only 25% of the year (if at all), these relatively low earnings are not surprising. Furthermore, among those subjects who had worked (N=1214), the mean duration of the job was short. During the year prior to intake, the average job lasted only about three months (98 days), and this did not vary by site, gender or referral source.**

The Most Recent Job

In their most recent job prior to intake, the average weekly wages for AYES subjects was $67 (this includes the $0 earned by subjects who did not work). While mean weekly wages varied significantly by site ($73 in Albuquerque, $68 in Miami, and $62 in New York),*** the absolute magnitude of the differences were small. The difference between males' mean weekly wage of $75 and females' of $53, was larger and statistically significant.****

Considering only those subjects who had a job in the year prior to intake, weekly earnings averaged $106. Differences among the sites were statistically significant, but small. Characteristics of the most recent job prior to intake are presented in Table 15. Weekly earnings were statistically independent of treatment (i.e., they were the same for experimentals and controls) and

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* $F(1,1386)=8.02$; $p=.0047$, controlling for site.

** In calculating mean job duration, the maximum number of days was set at 365; thus, no subject could work more than 100% of the year. For those subjects who had actually worked more than a year, therefore, the job duration was underestimated. Given the employment patterns of this sample, the problem of underestimation can be assumed to be minimal.

*** $F(2,2194)=5.20$; $p=.0056$.

**** $F(1,2194)=68.43$; $p<.001$, controlling for site.

TABLE 15

Characteristics Of Most Recent Job Before Intake, For Total Sample And By Site

(Number Of Cases In Parentheses)

	Total Sample	Alb.	Miami	New York	Significance Test	p
Mean Weekly Earnings	$ 106	$103	$110	$104	F(2,1411)=3.13	.04
	(1414)	(435)	(478)	(501)		
Mean Percent Taxes Not Deducted	20%	19%	19%	21%	X^2=7.4 df = 4	n.s.
	(1637)	(493)	(531)	(613)	Cramers V=.048	
Industry of Job	(1571)	(465)	(507)	(599)	X^2=138 df = 24 Cramers V=.209	.0001
Construction	7%	11%	6%	5%		
Manufacturing	12%	10%	12%	15%		
Retail Trade	27%	32%	30%	21%		
Business & Repair Services	9%	8%	6%	12%		
Professional Services	23%	17%	19%	31%		
Public Administration	6%	5%	10%	3%		
Other	16%	17%	17%	13%		
Occupation of Job	(1574)	(466)	(509)	(599)	X^2=69 df=20 Cramers V=.148	.0001
Professional, Technical, Kindred	6%	4%	4%	9%		
Managers & Adm.	1%	1%	1%	1%		
Sales	2%	3%	2%	2%		
Clerical	19%	16%	15%	25%		
Crafts	7%	7%	9%	5%		
Operatives	11%	8%	12%	12%		
Transportation Operatives	2%	2%	2%	2%		
Laborer (Non-Farm)	21%	21%	22%	21%		
Farm Laborer	-	1%	-	-		
Service	31%	36%	33%	25%		
Private Household	1%	1%	1%	-		
Type of Employer	(1628)	(493)	(531)	(604)	X^2=73 df=10 Cramers V=.15	.0001
Private Company	65%	68%	65%	65%		
Private Agency	2%	-	2%	2%		
Government	12%	16%	15%	6%		
Government Program	18%	14%	14%	25%		
Self-Employed	29%	2%	2%	1%		
Other	1%	-	2%	1%		

ethnicity. Males, however, had higher mean earnings ($111) than females ($94).* While criminal justice referrals had significantly higher mean weekly earnings ($109) than did non-criminal justice referrals ($103), this is probably attributable to the predominance of males among criminal justice referrals. These data are presented in Table 16.

About half the jobs were in retail trade or professional services (specifically in welfare services); however, there was significant variation of industry by site. (See Table 15 for details.) New York subjects were more likely to have been employed in professional services,** business and repair services, and manufacturing, but less likely to have jobs in retail trade. Albuquerque subjects were more likely to have construction experience, and Miami subjects to have had government jobs. Industry of the most recent job was independent of treatment and of referral source. Industry and gender were significantly related, however (X^2=82; df=12; p<.0001). Males were overrepresented in construction (9% of the males had construction jobs as compared to 2% of the females) and business services (10% to 5.5%) and underrepresented in retail (25% to 31%) and professional services (20% to 30%). Black subjects were more likely to have had professional service jobs than Hispanics (30% to 15%) and less likely to have had construction (4% to 11%) or retail jobs (23% to 30%).*** Almost a third of the jobs were in service occupations and about a fifth of each were clerical or

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* F(1,1414)=40.25; p<.0001

** "Professional Services" as a category describing types of industries includes a great variety of jobs ranging from very low-skilled, low-paying positions to those requiring considerable skill training and providing substantial renumeration. For example, "social services" would include jobs ranging from a temporary maintenance worker to a clinical psychologist. As one would expect, the people in our research sample, for the most part, held the lowest level jobs in the "Professional Services" category.

*** The difference in construction may be a site effect since there are almost no Blacks in the Albuquerque sample; on the other hand, in New York City labor market as a whole, Hispanics are overrepresented in construction.

TABLE 16

Mean Weekly Earnings of Most Recent Job Before Intake by Gender and Referral

	Mean Weekly Earnings		Number of Cases
Gender			
Male	$ 111		997
Female	94		419
	F(1,1414)= 40.25	$p<.0001$	
Referral Source			
Criminal Justice	$ 109		659
Other	103		756
	F(1,1413)= 5.19	$p=.0229$	

laborers jobs. New Yorkers were particularly likely to be clerical workers and less likely than subjects in the other two sites to be service workers.

Very few AYES subjects had jobs in management or sales. Six percent (9% in New York) had "professional" jobs; these were mainly positions as aides in social work agencies which are classified as professional due to the vagaries of the classification system.

Experimentals and controls did not differ significantly in occupational distribution. Males were significantly overrepresented among craft (e.g., construction and skilled maintenance) workers (9% to 2%) and laborers (28% to 5%) and underrepresented among clerical workers (12% to 36%). Black subjects were more likely than Hispanics to be clerical workers (22% to 14%) and less likely to be craft workers (5% to 10%). Criminal justice referrals were more likely to be laborers (24% to 18%) and less likely to be clerical workers (15% to 23%).

Type of employer (by site) is presented in Table 15. The most recent jobs before intake of about two thirds of the subjects at each site were with private companies. About 15% of these jobs in Albuquerque and in Miami were with government employers and another 15% were in government programs. In New York, on the other hand, only 6% had regular government jobs and 25% were in government programs.*

In summary, then, AYES subjects did not have impressive employment histories. Most had held a job at some time in their lives; however, their experience in the labor market was not good. Jobs were of short duration, and did not pay well. Even among those who had worked in the year before intake, only a quarter of the year was spent employed and total earnings were very low. Most of their jobs were in retail trade or service industries and in service, labor, and clerical occupations.

Criminal Justice System Involvement

Since the target population of the AYES project was "high risk youth," it was expected that a substantial proportion of the subjects would have prior juvenile or criminal justice system involvement. Furthermore, since one of the goals of the AYES project was to reduce such involvement, it was necessary to measure prior and subsequent illegal activities of AYES experimentals and controls. In the original design of the study two types of measures of such activity were planned: official records and self-reported illegal behavior.

As described in Chapter II above, official record data on all AYES subjects were collected for the two years prior to intake and the fourteen months subsequent to intake. In addition, the Vera intake interview contained 20 items designed to elicit reports from the subjects of the number of times they had participated in various illegal behavior during the previous 12 months. Analyses of these data, however, indicated that the rate of positive response was so low that it was concluded that the results were unreliable. As a result these data were dropped from the analyses, and the official records became the only measure of illegal activity of AYES subjects.

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* This could, however, be an artifact of coding practices rather than a real difference among the sites. If government jobs and government programs are combined, there are no differences among the sites, with approximately 30% of all AYES subjects falling into this employer category.

It is important to recognize that official records are not precise indicators of illegal behavior: some people are arrested for crimes they did not commit (or on charges that exaggerate the seriousness of the actual behavior); others are not caught for crimes they do commit. In addition, arrest and prosecutorial practices differ across jurisdictions and may differ across ethnic, racial, or socio-economic groups. Nonetheless, official records of criminal justice system involvement provide indicators of relative levels of illegal activities. That is, it is reasonable to assume that, within a site, AYES experimentals and controls would have similar experiences with the criminal justice system. Furthermore, while these data do not provide an accurate indication of how many crimes AYES subjects committed during any given period, they do allow for comparisons of experimentals and controls with respect to their criminal justice contacts prior to AYES intake and subsequent to AYES.

To understand the data, it is necessary to describe the jurisdictional structure of the three cities. In New York, with some exceptions for very serious offenses, a person is under the jurisdiction of the juvenile justice system (the Family Court) until his/her 16th birthday; individuals 16 and older are considered adults and come under the jurisdiction of the criminal court. In contrast, in New Mexico and Florida, the jurisdiction of the juvenile justice system extends to the 18th birthday.

Since AYES subjects ranged in age from 16 to 21 (with a median age of 18), a substantial proportion of those in Albuquerque and Miami came under the jurisdiction of the juvenile justice system for part or all of the two years prior to intake. In New York fewer than 25% of the AYES subjects were younger than 16 during the two years prior to intake. This distinction is important because juveniles are generally treated differently than adults; thus, for arrests on equivalent charges, the likelihood of prosecution, conviction, and incarceration is not likely to be the same for an adult as for a juvenile. In considering the records of AYES subjects in the three sites, therefore, these differences should be kept in mind. (There was no differentiation on the codesheets between information collected from adult records and that collected from juvenile records. With a small degree of error, the source of the data could be determined from the date of arrest and date of birth.)

Criminal history data going back two years prior to intake were collected on each individual in the AYES sample. These data included details on (up to) the five most recent arrests during the period; a count of the total number of arrests during the period; and a count of the total number of convictions during the period. In the pages that follow, the AYES sample is described in terms of number of arrests and convictions, and details of the arrest just prior to intake. In addition, differences between sites, males and females, criminal justice referrals and others are discussed.

Number of Arrests Pre-Intake

From an examination of the distribution of number of arrests in the two years prior to intake, it is clear that a majority of the AYES subjects had no record of arrests during this period. The number of pre-intake arrests, by site is presented in Table 17 below.

TABLE 17

Number of Pre-Intake Arrests by Site

	Albuquerque	Miami	New York
0	66.5%	64.6%	63.7%
1	18.6	14.8	17.3
2	7.1	6.7	8.9
3	3.6	5.3	5.6
4	1.5	3.7	2.4
5	2.6	1.8	0.7
6+	0.2	3.1	1.4
(N)	(609)	(757)	(807)

It can be seen from the data in Table 17 that only a very small proportion of the sample were arrested more than five times during the two years prior to intake; therefore, the detailed data generally represent the individuals' complete official record for the period. The table also shows that nearly two-thirds of the sample members in each site had no record of arrest during this period. Because of record sealing practices, it is not possible to conclude that only one-third of the sample was arrested during this period. Rather, we can conclude that one-third of the sample has an official record of arrest.*

An analysis of variance was computed on number of arrests; the variables included in the analysis were site, program model, gender, and referral source. Miami AYES subjects (experimentals and controls combined) had a mean of 0.92 arrests (N=757); New York subjects' mean was 0.75 (N=807); and Albuquerque's mean was 0.64 (N=609). Because the analysis of variance produced a significant effect for site (see Table 18), a Duncan Multiple Range test was performed to determine which of the three means was statistically different from the other. The results of the Duncan test indicated that the Miami mean was significantly greater (with alpha=.05) than the mean number of arrests for Albuquerque or New York.*

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* In all three jurisdictions there are regulations which assure the confidentiality of juvenile arrests and provide for the sealing of adult arrest records under specific circumstances. For example, in New York, if a criminal case involving a defendant over 16 years of age ends in a disposition favorable to the defendant, such as a decision not to prosecute or an acquittal, the court papers and arrest records are to be sealed, leaving no public record of the arrest ever having been made.

TABLE 18

Analysis of Variance on Number of Arrests Prior to Intake

Source	SS	df	F	p
Site (A)	27.72	2	7.68	.0005
Model (B)	53.32	3	9.85	.0001
Gender (C)	150.97	1	83.67	.0001
Referral (D)	515.77	1	285.86	.0001
A*B	19.68	6	1.82	N.S.
A*C	6.89	2	1.91	N.S.
A*D	32.83	2	9.10	.0001
B*C	15.00	3	2.77	.04
B*D	6.50	3	1.20	N.S.
C*D	22.50	1	12.47	.0004
Error	3873.86	2147		

NOTE: While it would be possible to test other effects (i.e., the 3- and 4-way interactions), only those listed in the table were tested.

In addition to the higher mean number of arrests for Miami subjects, the analysis of variance in Table 18 and the means in Table 19 (below) illustrate some important differences that are consistent throughout the analysis of pre-intake criminal justice system data.

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* While the analysis of variance and Duncan test indicate differences among the three sites, these analyses were based on a very large sample (N=2172). Since statistical significance is a function of sample size, it is important to consider whether this and other differences are substantively meaningful.

TABLE 19

Mean Number of Pre-Intake Arrests

Model	Albuquerque	(N)	Miami	(N)	New York	(N)
(A) By Site and Model						
1	1.02	(89)	1.41	(127)	0.85	(132)
2	0.59	(99)	0.45	(111)	0.66	(135)
3	0.56	(101)	0.89	(135)	0.54	(132)
Controls	0.57	(320)	0.90	(383)	0.81	(408)
(B) By Site and Gender						
Male	0.92	(361)	1.32	(492)	0.95	(588)
Female	0.23	(248)	0.18	(264)	0.21	(219)
(C) By Site and Referral Source						
Criminal Justice	1.37	(175)	1.61	(370)	1.07	(446)
Other	0.34	(434)	0.26	(386)	0.36	(361)

Model I participants in all three sites (but especially Miami) had more pre-intake arrests on the average than did participants in Models II and III. This suggests a systematic difference in participants who chose Model I over the other two models. An analysis of the effect of assignment type (guided choice vs. random) and its interaction with model indicated that Model I participants, regardless of assignment type, had more arrests on the average than did Model II or III participants. While guided choice participants had a higher mean number of arrests (.88) than did those who came in under random assignment (.62), this simply reflects the greater proportion of CJ referrals who came into AYES during the guided choice period.

One major difference between Model I and the other two models is the preponderance of males; 78% of the Model I participants were male, as compared to 61% of Model II and 58% of Model III participants. The data in Table 19 (and the significant effect for gender* demonstrate that males have significantly more arrests (mean=1.07) than females (mean=.21); thus the Model I effect may be partially attributable to gender. Similarly, criminal justice referrals (mean=1.32) have significantly more arrests ($F(1,2147)=285.86$, $p<.0001$) than do non-criminal justice referrals (mean=0.32); and criminal justice referrals are overrepresented in Model I (54% as compared to 39% in Model II and 45% in Model III). Neither gender nor referral source totally explains the difference among models, however; for example, criminal justice referrals in Model I had an average of 1.63 arrests, as compared to a mean of 1.07 arrests for CJ referrals in Model II and 1.26 for those in Model III. (Similarly male Model I participants have more arrests on the average than males in the other two models.) These effects are consistent throughout the analyses; for example, on the arrest just prior to intake, males tend to have been arrested on more serious charges than females, and criminal justice referrals are more likely to have been arrested on felony charges than non-criminal justice referrals.

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* F(1,2147)=83.67, p<.0001).

Number of Convictions Prior to Intake

Similar analyses were conducted on the number of convictions prior to intake. As would be expected, the average number of convictions is substantially lower than the average number of arrests. The distribution of number of pre-intake convictions is presented in Table 20. The mean number of pre-intake convictions for subjects in New York was 0.52; in Miami, 0.41; and in Albuquerque, 0.29. The results of the Duncan test indicate that the mean for New York is significantly greater than that for Miami, which is significantly greater than that for Albuquerque.

By comparing the arrest data to the conviction data, it can be seen that while Miami had the highest mean number of arrests, New York had the highest mean

TABLE 20

Number of Pre-Intake Convictions by Site

	Albuquerque	Miami	New York
0	80.3%	78.1%	68.8%
1	13.2	11.1	19.0
2	3.8	7.0	7.4
3	1.8	2.4	3.0
4	0.3	0.7	0.8
5	0.3	0.4	0.3
6+	--	0.5	0.6
(N)	(602)	(754)	(793)

number of convictions. This may partially reflect the difference in age of jurisdiction. That is, a 16 year old is a juvenile in Miami, while the same 16 year old is an adult in the New York courts, and the courts may be more reluctant (or less able) to convict a juvenile (in Family Court) than an adult (in criminal court).* Additionally, the New York convictions include persons found guilty of violations. In New York State, a person who has been convicted on a violation has not been convicted of a crime; thus the existence of the violation category in New York State increases the impetus to plead guilty. Since New Mexico and Florida do not have this charge category, the data may not be comparable across sites. (It can be seen from the discussion (below) of conviction charges on the most recent arrest that approximately one-third of these convictions in New York were on violations. If one-third of all convictions in the New York site were on violations, the percent of New York AYES subjects with convictions on felony or misdemeanor charges (21.6) would be approximately equal to the 21.9% of Miami subjects and 19.7% of Albuquerque subjects who were convicted of at least one crime.)

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* Among those AYES subjects with at least one arrest prior to intake (N=742), there was a very weak correlation (r=.16) between number of convictions and age at intake. Thus, it is likely that there are other factors influencing the conviction rate in New York.

Data on convictions may be analyzed in two ways: either including all cases, or by excluding those individuals who had never been arrested, and therefore, could not be convicted. Since the utility of including non-arrested individuals in an analysis of convictions is limited, only the analyses of arrested persons is presented. This analysis produced significant effects for site, gender, and referral source. The means for the significant effects on number of convictions are presented in Table 21, and the results of the analysis of variance are in Table 22. This analysis is consistent with the results of the analyses on arrests. Specifically, males and criminal justice referrals were arrested and

TABLE 21

Mean Number of Pre-Intake Convictions

Site	Mean	(N)
Albuquerque	0.89	(197)
Miami	1.15	(266)
New York	1.48	(279)
Model		
1	1.25	(153)
2	1.14	(96)
3	1.09	(118)
Control	1.24	(375)
Gender		
Male	1.27	(649)
Female	0.73	(93)
Referral Source		
Criminal Justice	1.30	(537)
Other	0.97	(205)

convicted more often than females and non-criminal justice referrals; male criminal justice referrals, on the average, have the highest number of arrests (mean=1.49) and the highest number of convictions (mean=1.35).

TABLE 22

Analysis of Variance on Number of Convictions Prior to Intake

Arrested Cases Only	SS	df	F	p
Site (A)	41.06	2	14.28	.0001
Model (B)	2.61	3	0.61	N.S.
Gender (C)	13.98	1	9.73	.002
Referral (D)	8.78	1	6.11	.02
A*B	5.48	6	0.64	N.S.
A*C	6.82	2	2.37	N.S.
A*D	4.17	2	1.45	N.S.
B*C	8.09	3	1.88	N.S.
B*D	1.75	3	0.41	N.S.
C*D	0.13	1	0.09	N.S.
Error	92.88	717		

Arrest Just Prior to Intake

To describe the criminal justice experiences of the AYES subjects, a series of analyses were computed on the arrest just prior to intake. Each of the details on that arrest (severity of arrest charge; type of crime; disposition; conviction charge severity; and type of conviction charge) was cross-tabulated with program model, treatment (experimental or control), referral source, gender, and ethnicity. These analyses were run separately for each site for only those subjects who had been arrested prior to intake. Analyses on type and severity of conviction charge were computed only on those subjects who had received a conviction on the arrest just prior to intake.

As would be hoped (for the sake of comparability), in all three sites experimentals and controls were equally likely to have been arrested prior to intake (See Table 23). Furthermore, there were no significant differences between experimentals and controls on any of the variables associated with the arrest just prior to intake, with the exception of one analysis. Among the arrested subgroup in New York, experimentals were more likely than controls to be found guilty, but not incarcerated ($X^2=9.55$; df=2; p=.0084). If one looks at all members of the New York sample, arrested and non-arrested, however, it can be seen that 73% of the experimentals were either not guilty or not arrested, as compared with 76% of the controls. Therefore, this difference does not appear to be important to this research.

TABLE 23

Percent Arrested Prior to Intake by Site and Treatment

	Site			
Treatment	Albuquerque	Miami	New York	TOTAL
Experimental	31%	35%	34%	33%
(Total N)	(295)	(376)	(411)	(1082)
Control	29%	32%	38%	35%
(Total N)	(323)	(393)	(421)	(1137)

Severity. Severity of charge for the arrest just prior to intake was coded as felony or misdemeanor. Cross-tabulations indicated significant relationships (in all three sites) between referral source and severity of arrest charge. Even after taking into account that criminal justice referrals were more likely to be arrested, the data indicate that among the arrested subsample, criminal justice referrals were more likely than others to be arrested on felony charges. (See Table 24.) Given the definition of a criminal justice referral (see Chapter II above) as a person referred directly or indirectly by an agency of the criminal justice system, one would expect CJ referrals to have been arrested more often, on more serious charges, leading to more convictions, and hence, relationship with some criminal justice agency.

TABLE 24

Severity of Pre-Intake Arrest Charge by Referral Source

SITE

Referral Source	Albuquerque		Miami		New York	
	Felony	Misd.	Felony	Misd.	Felony	Misd.
Criminal Justice	31.4%	68.6%	80.4%	19.6%	74.8%	25.2%
(N)	(32)	(70)	(86)	(21)	(172)	(58)
Other	15.5%	84.5%	60.6%	39.4%	61.8%	38.2%
(N)	(13)	(71)	(20)	(13)	(42)	(26)

It should also be clear from Table 24 that all analyses on these data must include site as a variable. It is likely that differences in arrest and charging practices and statutory classifications are responsible for the substantially lower percentage of felony arrests in Albuquerque (24%, overall) than in Miami (76%) and New York (72%).

The other variable related to severity of arrest charge is gender; in Albuquerque and New York, males were more likely than females to be arrested on felony charges. Again, this effect is above and beyond the greater likelihood for males than females to be arrested. (See Table 25.)

TABLE 25

Severity of Pre-Intake Arrest Charge by Gender

Gender	Albuquerque		Miami		New York	
	Felony	Misd.	Felony	Misd.	Felony	Misd.
Male	27.6%	72.4%	77.0	23.0	73.4	26.6
(N)	(40)	(105)	(94)	(28)	(201)	(73)
Female	12.2%	87.8%	66.7	33.3	54.2	45.8
(N)	(5)	(36)	(12)	(6)	(13)	(11)
	X^2=4.12 p=.0422 phi=.149		X^2= .92 N.S.		X^2=4.02 p<.005 phi=.116	

It is clear from the data in Table 25 that the relationship between gender and severity of arrest charge is weak (phi=.149 in Albuquerque and .116 in New York), but consistent across sites. Furthermore, the number of females arrested in each site is quite small, rendering comparisons difficult. There were no significant relationships (for the arrested sample) between model or ethnicity and severity of arrest charge. As was indicated above, however, Model I participants were more likely to have been arrested than were participants in Model II or III.

Disposition. Crosstabulations were run of treatment, model, referral source, gender, and ethnicity with disposition of the arrest just prior to intake. Possible dispositions were not convicted (including dismissal of the charges), convicted without incarceration, and convicted with incarceration. The only significant relationship in the Albuquerque AYES sample was with gender ($X^2=6.39; df=2, p<.05$). While 20% of the arrested males and 27% of the arrested females were found not guilty, 18% of the males as compared to none of the females were incarcerated. This is not surprising since males were arrested more often and on more serious charges.

There were no significant relationships with case disposition in Miami. In New York there was a significant relationship between referral source and disposition ($X^2=5.71; df=2, p=.0575$). As was indicated above, criminal justice referrals were more likely to have been arrested, more likely on felony charges, and once arrested, were more likely to be found guilty and incarcerated. (See Table 26.)

TABLE 26

Pre-Intake Case Disposition by Referral Source (New York)

Disposition	Criminal Justice	Other
Not Convicted	18.5%	30.7%
Convicted, No Incarceration	54.4%	53.2
Convicted, Incarceration	27.2%	16.1
(N)	(206)	(62)

Conviction. In all three sites the criminal justice referred clients, who were more likely than others to have been arrested on felony charges, were also more likely to be convicted on felony charges. Conviction charges were coded as felony, misdemeanor, and violation to allow for reduction in severity from arrest to conviction; however, as discussed above, the category of violation exists only in New York. The conviction charge severity data are presented in Table 27, which demonstrates how difficult it is to compare across sites. As was indicated above, members of the Albuquerque AYES sample were less likely than those in Miami or New York to have been arrested on felony charges. The conviction data are consistent with that finding; while one out of three convictions of criminal justice referrals in Albuquerque were on felony charges, two out of three in Miami were on felony charges. New York had the highest conviction rate, but the lowest on felony charges (for criminal justice referrals). The additional category in New York (violation) may partially explain the high conviction rate -- the New York criminal justice system allows for convictions on non-criminal offenses. Despite the differences among the sites, the direction of the relationship between referral source and severity of conviction charge was consistent: criminal justice referrals were more likely than others to have been convicted on felony charges. This relationship was strongest in Miami (phi=-.41), where 64.4% of the convicted criminal justice referrals, as compared to 16.7% of the others, were convicted on felony charges.

Consistent with the finding that Albuquerque males were more likely than females to be found guilty and incarcerated, convicted males were more likely than convicted females to have been convicted on felony charges (X^2=5.734; p=.0166; phi=.24). Of the 79 convicted males, 23 (29%) were convicted on felony charges, while of the 22 convicted females, only 1 (5%) was convicted on a felony.

TABLE 27

Severity of Pre-Intake Conviction Charge by Referral Source*

	Albuquerque		Miami		New York		
	Felony	Misd.	Felony	Misd.	Felony	Misd.	Violation
Criminal Justice	36.5%	63.5%	64.4%	35.6%	25.6%	51.2%	21.2%
(N)	(19)	(33)	(38)	(21)	(40)	(33)	(33)
Other	10.2%	89.8%	16.7%	83.3%	10%	45%	45%
(N)	(5)	(44)	(3)	(15)	(4)	(18)	(18)
	$X^2=9.66; p<.002$ phi=-.31		$X^2=12.63; p=.0004$ phi=-.41		$X^2=10.84; p=.0044$ Cramer's V=.24		

* Percentages are of convicted members of the AYES sample.

Type of Crime. Arrest and conviction charges were categorized according to two schemes: (1) violent, property, other* and (2) income producing, non-income producing.** Thus categorized, each of the four resulting variables was crosstabulated with treatment, model, referral source, gender, and ethnicity.

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* Crime categorizations were (1) violent: homicide, rape, robbery, assault, arson; (2) property: burglary, larceny, motor vehicle theft, forgery, counterfeiting, fraud, embezzlement, (buying, receiving, or possessing) stolen property, vandalism; (3) other: obstructing justice, weapons, prostitution and commercialized vice, drugs, gambling, disorderly conduct, driving while intoxicated, misconduct, juvenile traffic offenses, adult (criminal) traffic offenses.

** Crimes were categorized as (1) income producing: robbery, burglary, larceny, motor vehicle theft, arson, forgery, counterfeiting, fraud, embezzlement, (buying, receiving, or possessing) stolen property, prostitution and commercialized vice, gambling and (2) non-income producing: homicide, rape and other sex offenses, assault, obstructing justice, vandalism, weapons, disorderly conduct, misconduct, driving while intoxicated, juvenile traffic offenses, adult (criminal) traffic offenses. Drug offenses were excluded from this variable because in many states, including New York, possession of more than a given quantity is charged as sales. Therefore, it is not possible to distinguish between drug possession (non-income producing) and drug sales (income producing).

The only variable that produced any consistent results was referral source.

There were significant relationships between referral source and the type of crime for which the individual was convicted in both Miami and New York. In both sites convicted criminal justice referrals were more likely than others to be convicted for income-producing crimes. This relationship did not exist in Albuquerque. The data are presented in Table 28.

In addition, convicted Miami criminal justice referrals were more likely than others to have been convicted for violent crimes (25% vs. 14%) or property crimes (52% vs. 19%). This is not surprising since many income-producing crimes are either violent (e.g., robbery) or property crimes (e.g., burglary or larceny).

TABLE 28

Type of Pre-Intake Conviction Charge by Referral Source*

	Albuquerque		Miami		New York	
Referral	Income	Non-Income	Income	Non-Income	Income	Non-Income
Crim.Just.	24.5%	75.5%	67.0%	33.0%	57.4%	42.7%
(N)	(12)	(37)	(65)	(32)	(78)	(58)
Other	27.7%	72.3%	38.9%	61.1%	27.5%	72.5%
(N)	(13)	(34)	(7)	(11)	(11)	(29)
	X^2=0.125; NS		X^2=5.13; p=.0235 phi = .21		X^2=11.02; p=.0009 phi = .25	

* NOTE: Percentages are of convicted cases only.

Summary of Criminal Justice System Involvement.

Although the target population of the AYES program was high risk youth, the research subjects were by no means "hard core" criminals. Nearly two-thirds of the sample had no official record of arrest in the two years prior to AYES intake, and 75% of the sample had no convictions during that period. In addition, nearly half of those with an arrest record had been arrested only once. Thus, although one of the goals of the AYES project was to reduce criminal justice system involvement, most sample members had little or no prior contact with the system. This characteristic of the sample limits the potential "effectiveness" of the AYES program on criminal justice system outcomes and should be considered when evaluating the data presented in Chapter VI.

CHAPTER IV: STANDARD ASSESSMENT SYSTEM OUTCOMES

As was described in Chapter II, the set of instruments known as the Standard Assessment System (SAS), designed by ETS, was implemented by the AYES project. A battery of seven scales, part of the SAS, was administered at AYES intake (pre-test) and exit (post-test); these scales measured vocational attitudes (VA), job knowledge (JK), job holding skills (JH), work-related attitudes (WR), job seeking skills (JS), sex role stereotypes of adult occupations (SS), and self-esteem (SE). Using analysis specifications provided by ETS, these data were analyzed for scale reliability, initial and post-program differences between experimentals and controls (within site), and gains from intake to exit.

Reliability

Although the primary purpose of these scales was to serve as short-term program outcome measures (gains), before using them as such, it was necessary to determine whether they were statistically reliable. A scale may be considered reliable if the items that compose it are shown to be measuring the same construct. In other words, before one can sum across a set of items which purport to measure some psychological characteristic (e.g., self-esteem), it is necessary to demonstrate that the items used do, in fact, measure the same thing. Such an analysis does not indicate whether the scale is a valid measure of that construct; only that it measures a single construct. Thus, for example, it would be possible to develop a highly reliable scale that the researcher intended to measure self-esteem, but that actually measured reading ability. Since these data were being collected essentially for ETS' purposes and since the validity of the scales was tested by ETS, our analyses focused on scale reliability. This was necessary because, prior reliability tests notwithstanding, each time a scale is used with a new population (e.g., high risk youth), it is necessary to test its reliability for that population.

Split-half reliability analyses were computed for each measure. Very simply, split-half reliability indicates the extent to which two halves of the scale are correlated. (For further discussion of psychometric reliability, see Nunnally, 1967.) A reliability coefficient can range from zero (indicating that there is no relationship among the items) to one (indicating perfect correlation among the items). The value which the coefficient must reach before a scale is accepted as "reliable" depends in part on the purpose to which the scale is put. So, for example, if a scale is being used to determine an individual's job eligibility, fairness dictates that the scale have a very high coefficient. Somewhat lower levels are generally acceptable for research studies. For the purposes of this study, a reliability coefficient of .60 was considered acceptable.

It must be noted that reliability is a function of the number of items in the scale; thus, a 30-item scale will have higher reliability than a 15-item scale with the same average inter-item correlations. Since the number of items varies among the SAS scales, this should be kept in mind when comparing reliabilities.

Split-half reliability was computed on each scale by site and treatment (experimental/control). The analysis of each scale was done separately so that an individual with missing data on a particular scale was eliminated from the analysis of that scale; therefore, the sample sizes vary slightly among the scales.

The split-half reliabilities, presented in Tables 29 A & B, were generally high (above .60); the "halves," in this case, were odd/even-numbered items. It is clear from Table 29 that the lower reliabilities were on those scales with relatively few items (e.g., Job Holding Skills, with 11 items). While the especially low reliabilities on JH in Albuquerque are cause for concern, it is possible, to some extent, to correct for unreliability when using the scales in an analysis. In general, there is remarkably little variation in reliabilities among the sites, or between experimentals and controls. In fact, the pre-test and post-test reliability scores are quite consistent for the more reliable scales. The reliability scores for the less reliable scales, such as JH and SE, tend to be higher in the post-test period. This may be a function of better test administration or more participant familiarity with the test.

TABLE 29(A)

Split-Half Reliability - Pre-Test

Scale	# of Items		Albuquerque		Miami		New York	
			EXP	CON	EXP	CON	EXP	CON
VA	30	N	280	289	345	352	376	364
		r_{ii}	.72	.77	.76	.78	.70	.75
JK	30	N	283	301	334	344	360	371
		r_{ii}	.63	.69	.76	.71	.73	.69
JH	11	N	284	313	345	364	384	393
		r_{ii}	.46	.30	.56	.55	.49	.61
WR	16	N	287	314	344	359	375	389
		r_{ii}	.65	.69	.62	.63	.68	.67
JS*	17	N	278	297	292	310	351	368
		r_{ii}	.61	.70	.70	.79	.76	.75
SS	21	N	291	319	359	375	389	393
		r_{ii}	.88	.86	.80	.81	.82	.82
SE	15	N	289	310	337	348	370	381
		r_{ii}	.56	.54	.55	.61	.64	.47

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* The smaller sample sizes for this scale are indicative of the fact that it is necessary to read English to answer these items. Although the administrator was to read each item of the SAS to the subjects (in English, Spanish, or Creole), this test measured job seeking skills, one of which is the ability to read English.

TABLE 29(B)

Split-Half Reliability - Post-Test

Scale	# of Items		Albuquerque		Miami		New York	
			EXP	CON	EXP	CON	EXP	CON
VA	30	N	197	157	237	149	186	154
		r_{ii}	.77	.73	.78	.75	.72	.72
JK	30	N	186	152	212	146	166	144
		r_{ii}	.70	.71	.75	.75	.78	.68
JH	11	N	195	150	239	144	179	160
		r_{ii}	.79	.59	.71	.74	.74	.68
WR	16	N	203	161	236	151	180	159
		r_{ii}	.73	.76	.67	.64	.71	.64
JS	17	N	191	150	206	137	170	152
		r_{ii}	.78	.72	.80	.83	.80	.80
SS	21	N	206	161	251	156	185	163
		r_{ii}	.89	.86	.81	.77	.82	.82
SE	15	N	206	153	239	154	181	157
		r_{ii}	.61	.73	.55	.68	.70	.46

Analysis of Change from Pre-Test to Post-Test

To test for program effects on the attitudes and skills measured by the scales in the SAS, relative change from intake to exit for experimentals was compared to that for controls for each scale. The simplest approach to such an analysis is to subtract (for each person who took both the pre- and post-tests) the score obtained at intake on a given scale from the score obtained at exit on that same scale. For example, a person with a score on the Vocational Attitudes scale of 12 at intake and 15 at exit would have a gain score for VA of 3. If this is done for each scale, it is possible to compare the average gain for experimentals to the average gain for controls.

Mean pre- and post-test scores for each scale along with the variance associated with those means were computed for each scale in each site. The results are presented for experimentals and controls in Table 30. It is clear from the data in the table that there were virtually no changes in the means from intake to exit. Furthermore, for those scales on which there was any increase over time, the magnitude and direction of change was the same for experimentals and controls. For example, the mean on VA for Albuquerque Experimentals increased from 19.57 to 21.93, and the mean for Albuquerque Controls increased from 19.68 to 21.69. Thus, this analysis suggests that the program had no discernible impact on participant performance with respect to these scales.

In pursuing this question further, an analysis of variance using the combined scores from the three sites was computed on gain scores for each of the seven scales. The only independent variable in these analyses was treatment (experimental/control). These analyses showed a highly significant effect for treatment on the JK gain score, $F(1,1113)=11.86$, $p=.0006$, and on the JH gain score, $F(1,1140)=8.20$, $p=.004$. The analysis also revealed nearly significant ($p=.07$) effects of treatment on the WR gain score, $F(1,1109)=3.27$ and on JS gain, $F(1,1128)=3.16$. It is important to note, however, that for the largest effect (on Job Knowledge), despite the significance level of .0006, the amount of variance explained by treatment is only 1%. With sample sizes of over 1000 (subjects who completed both the pre-test and post-test), significant effects are easily obtained. In this case, therefore, statistical significance does not guarantee that the effects are meaningful.

These analyses of raw gain scores did not take into account the possible relationship between pre-test score and post-test score, nor did they consider possible differences between treatment groups at intake. Thus, for example, if experimentals had higher scores at intake than controls, raw gain score analysis

TABLE 30
Scale Means And Variances

	EXPERIMENTALS				CONTROLS			
	INTAKE		EXIT		INTAKE		EXIT	
	Mean	Var.	Mean	Var.	Mean	Var.	Mean	Var.
ALBUQUERQUE								
VA	19.57	17.90	21.93	21.70	19.68	19.57	21.69	17.31
JK	22.49	9.70	22.10	14.06	22.31	10.25	22.12	10.00
JH	31.02	3.5	30.57	8.99	30.95	3.74	30.71	6.55
WR	48.43	40.41	47.80	49.24	48.43	41.70	49.39	50.25
JS	11.96	6.50	11.54	12.31	11.97	7.40	11.43	9.94
SS	46.39	65.61	46.90	86.29	45.31	62.18	46.51	65.35
SE	35.98	9.36	35.80	13.04	36.10	8.53	35.83	14.63
MIAMI								
VA	18.07	23.30	21.93	21.70	17.89	23.72	18.60	23.03
JK	19.97	18.10	19.62	17.20	19.49	17.53	19.33	19.86
JH	29.99	6.71	28.97	13.96	29.73	7.65	28.82	12.76
WR	46.35	48.84	45.46	49.60	44.94	48.51	44.73	48.80
JS	10.60	10.60	10.00	14.79	10.12	12.85	9.45	15.76
SS	44.68	58.87	45.11	65.53	44.24	61.98	44.05	56.72
SE	35.99	11.08	34.94	18.09	35.54	12.20	24.70	17.41
NEW YORK								
VA	19.18	18.57	20.53	20.54	18.90	20.30	19.75	18.40
JK	21.24	15.42	21.15	17.64	20.80	16.08	20.94	15.49
JH	29.83	6.45	29.53	12.60	29.29	8.93	29.29	10.89
WR	45.95	44.81	46.71	50.81	45.86	47.09	46.63	45.64
JS	11.28	11.03	11.23	15.80	10.78	12.48	11.13	13.51
SS	43.83	58.17	44.29	65.38	43.31	61.27	44.00	61.00
SE	35.45	13.61	35.88	15.75	35.34	12.91	35.16	15.34

would not adjust for this difference. Another possibility, that is not covered by raw gain score analysis, is that the program was more effective for those participants who came into it with relatively high scores than it was for those who came into it with relatively low scores.

Analysis of covariance, on the other hand, considers changes within a treatment group which are related to initial status, and corrects for such a relationship. So, a series of analyses of covariance were conducted to determine whether experimentals show relatively higher post-test scores when controlling for pre-test score, educational level, gender, ethnicity, etc. In each analysis treatment was the independent variable and pre-test score, gender, ethnicity, reading (STEP score) and educational levels served as covariates. The post-test score served as the dependent variable. Once again, although we found highly significant effects for the independent variable and most of the covariates, they accounted for very little variance.

For example, pre-test score, gender, ethnicity,* and reading level all reached the .001 significance level with post-test Vocational Attitude (VA) score as the dependent variable. However, as shown in Table 31, the effect size measures (eta^2) for the treatment variable and most of the covariates are very small. Thus while the independent variable and the covariates taken together account for 46% of the variance in VA post-test score, the pre-test VA score alone accounts for 38% of the variance. This pattern is evident for each of the seven post-test measures; virtually all of the explained variance is attributable to the pre-test score. Thus, the higher an AYES subject scored on a given pre-test measure, the higher he/she was likely to score on that same measure at post-test.

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* Since only 2% of the Miami and New York samples were white, these subjects were eliminated from analyses involving a measure of ethnicity. Thus the variable "ethnicity" was defined to be Black or Hispanic. Black was given a score of 1 and Hispanic a score of 0 on the dummy variable.

TABLE 31

Eta² For Treatment And Covariates
On Post-Test Scores

	VA	JK	JH	WR	JS	SS	SE
TREATMENT	.001	.002	0	0	0	.001	.002
PRE-TEST*	.38	.35	.23	.38	.42	.34	.15
ETHNICITY	.02	.002	.002	.008	0	.005	.004
GENDER	.017	.008	.014	.006	.009	.05	.02
STEP	.02	.019	.033	.02	.04	0	.03
ED. LEVEL	.001	0	0	0	0	0	.001
R^2	.46	.39	.29	.42	.48	.36	.21

* For each dependent variable (post-test), the appropriate pre-test score was used as a covariate. Thus, for example, in predicting Job Seeking Skills post-test score, the Job Seeking Skills pre-test score was the covariate.

Knowing whether the subject was male or female, Black or Hispanic, or an experimental or control subject would not add any meaningful information about how that person was likely to score on the post-test. These analyses again fail to show any meaningful effect of the program on the participants' test scores.

There was some concern that relationships between post-test scores and demographic variables would be obscured by the covariance analysis. For example, if gender were related to Job Knowledge (JK), then the relationship between gender and the JK score at exit would be obscured by the covariance analysis, since the variance explained by pre-test score would likely include that due to gender. To examine this possibility, correlations werre computed (by treatment) of gender, ethnicity, and education level with the seven pre-test scores. While correlations were computed separately for experimentals and controls, there were no real differences between the two groups. Therefore, the correlations presented in Table 32 are for the sample as a whole.

It can be seen from the table that the correlations of the covariates with the pre-test scores are relatively low, and it is unlikely that these relationships have affected the results of the covariance analysis. Despite the generally low correlations, some distinct patterns emerged. The correlations of gender with the pre-tests were consistently positive, ranging from .08 for JK to .26 for JH. This implies that females were likely to obtain slightly higher pre-test scores than were males. In addition, the correlations of ethnicity with the pre-tests were consistently negative (though quite small), indicating a tendency for Hispanics to obtain higher scores than Blacks. Finally, education level was positively correlated with the pre-test scores, with the highest correlation being that of education level with Job Seeking Skills (r=.27). The Job Seeking Skills scale is the one most closely tied to English reading ability; the respondent is required to read job advertisements and application forms to determine the correct answers to the questions.

The correlations of education level, ethnicity, and gender with the pre-tests cannot be considered independently, however. These variables are all related to site, with Albuquerque having the highest proportion of Hispanics, the highest proportion of females, and the highest education level.* Furthermore, the correlation between gender and education level is .13. (The correlations between ethnicity and education level (r=.02) and between ethnicity and gender (r=-.01)

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* The correlation between a dummy coded site variable (1=Albuquerque/0=Miami or New York) and education level is .23, and the correlation of the site variable with ethnicity is -.61.

TABLE 32

Correlations Of Covariates With Pre-Test Scores

	VA	JK	JH	WR	JS	SS	SE
Gender*	.12	.08	.26	.12	.17	.18	.15
Ethnicity**	-.07	-.14	-.16	-.14	-.02	-.02	-.04
Ed. Lev.	.17	.19	.13	.19	.27	.11	.14

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* Gender was coded: 1=male, 2=female.

** Ethnicity was coded: 0=Hispanic, 1=Black; therefore, negative correlations indicate that Hispanics tend to score higher than Blacks.

are not significantly different from zero.) Thus, if site or education level were controlled, the relationships of the pre-test scores with ethnicity and gender would probably be even weaker.

Job Aspirations

The subjects' job aspirations at exit were used as an additional short-term measure of program effect; a positive difference between experimentals and controls in the expected direction could be interpreted to mean the AYES program had a positive effect on participants' job aspirations. Job aspirations were measured using the question, "What kind of full time job would you like best right now?" (This question appeared in the Program Completion Survey and Control Group Status Survey, administered at the time of the exit interview.) The responses to this question were coded on a scale of 1-5 provided by ETS, intended to be a measure of relative status of the job to which the respondent aspired.

A hierarchical regression analysis was run on job aspiration scores, using pre-test Vocational Attitudes (VA) and Work-Related Attitudes (WR), gender, ethnicity, educational level, and treatment as predictors. Treatment was entered last in the equation to assess program effects while controlling for the other variables (which existed prior to entry into the AYES research sample). The set of predictor variables accounted for 10% of the variance in job aspirations, with gender accounting for half of the explained variance. Ethnicity, education level, and treatment accounted for no variance in job aspirations. Thus, there was no indication from this analysis that either of the two attitudinal measures or the AYES program had any effect on the subjects' job aspirations.

In addition, a series of seven separate regressions were computed using the pre- and post-test scores on each of the scales as predictors of job aspirations. The highest R^2 for any of these regressions was .04. Similarly, a regression using the seven pre-test scores as predictors produced an R^2 of .07, and an equation using the seven post-test scores as predictors of job aspirations produced an R^2 of .05. These analyses suggest that the pre- and post-test scores obtained from the SAS do not predict AYES respondents' job aspirations.

Conclusions

The lack of treatment effects on either post-test scores or job aspirations need not be considered a failing of AYES. The AYES program focused on changing behavior (i.e., improving employment and criminal justice outcomes), not on changing attitudes. The huge body of social psychological literature on resistance to attitude change suggests that such change would be unlikely as a result of a six-month job training program.

In addition, while the reliabilities of the SAS scales were generally acceptable, the AYES research staff raised questions about the validity of the scales. Some of the scales appeared to lack face validity, and many did not seem appropriate to this population. Thus, there is the additional possibility that the lack of difference on the post-test scores between AYES participants and controls is more a function of the psychometric properties of the scales than it is a reflection of the quality of the AYES program. For this reason, we made very little use of the SAS variables in our subsequent analyses of the employment and criminal justice impacts of the program.

Additional Note

As was indicated in Chapter II, the SAS consisted of the pre- and post-test measures, the IPP, and interviews conducted at the time of the exit, three-month follow-up, and eight-month follow-up. The analyses presented in this chapter were computed on variables constructed from the pre- and post-tests, and demographic data collected on the IPP. The analyses in the chapters that follow were based primarily on data collected in the Vera Intake, Exit, and Eight-Month Follow-up interviews. Data on program participation and termination status were obtained from the IPP, as were the demographics. The data on the SAS interviews were not analyzed for this report. This decision was based on resource constraints (primarily, time) and on the belief that the data from the Vera interviews were inclusive of that on the SAS interviews while also being more complete and accurate. For example, both the Vera and the SAS interviews contained questions about the respondents' jobs. In the SAS interviews, occupation was coded on a scale of 1-5, while the occupation data on the Vera interviews permitted us to describe occupation in terms of a much more sophisticated three-digit occupation code developed by the U.S. Census Bureau. In addition, the SAS interviews contained detailed information on the most recent job during a given period, while the Vera interviews contained detailed information on the most recent job and the prior job during a period. Thus, the analyses in the chapters that follow present a more complete picture of the employment outcomes of the AYES research subjects than could have been obtained from the SAS data.*

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* Although the SAS interview data were not analyzed for this report, complete data tapes were submitted to ETS according to the specifications they provided. Thus, it would be possible for ETS to merge the AYES data with those of the other projects which implemented the SAS, and conduct analyses comparing the outcomes of the various programs.

CHAPTER V: EMPLOYMENT OUTCOMES

The focus of this chapter is on employment outcomes of the AYES program. Major employment variables include: whether the subject obtained employment; the average weekly earnings of AYES subjects in their most recent post-program jobs; the percent of time employed (between exit and the 8-month follow-up); and the total earnings during the period from exit to 8-month follow-up. All of these variables were constructed from data collected on the Vera 8-Month Follow-up interview and, thus, are available only for those subjects who received an 8-month follow-up.

The data analysis followed the strategy described in Chapter II; that is, the first level of analysis involved tests for differences between experimentals and controls on the employment variables described above; these results are presented in Section A below. The second level of analysis, in Section B, pertained to differences among the program models. Finally, the third level of analysis involved variables outside the AYES experimental design. In this chapter, the third level is developed rather elaborately and focuses primarily on testing hypotheses derived from competing theories of labor market structure. These are presented in Section C.

These theories are particularly relevant to the AYES research because the program rests on the assumption that the employment experience and future prospects of the participants can be improved by improving their stock of human capital. That is, the AYES program was designed to increase the employability of the participants by increasing skills (including one's knowledge of and commitment to appropriate behavior on a job, as well the basic reading, math and verbal skills) necessary to obtain and keep a job. Human capital theory views such skills, education, and knowledge as investments which, because they increase the productivity of the worker, will earn a return in terms of increased employment possibilities and higher wages. The basic hypothesis of the AYES research is that participants, because of their increased skills and understanding of the world of work, should have more positive employment outcomes than the control group members. Lack of differences on employment outcomes of experimentals and controls could be attributed to failure of the program to provide such improved skills and enhanced understanding.

Alternatively, segmented labor market theorists would contend that the labor market is divided into two different segments: the primary segment, in which workers receive returns from their investments in human capital, and the secondary segment, in which differences among workers have little or no impact on their earnings or job quality. Because the AYES subjects are poor, minority, under-educated, and young, the jobs they are able to obtain, given the structure of the labor market, are likely to be within the secondary segment. Thus, there would be no reason to expect experimentals to find better jobs than controls. These competing theories will be discussed, and results of tests of the hypotheses generated by the two theories will be presented below.

A. The Effects of Treatment: Differences between Experimentals and Controls

The employment-related objectives of the AYES program included: increasing participants' employment and earnings, and improving their ability to secure and retain employment. The educational and training services were expected to raise participants' skill levels; the vocational counseling was expected to increase their knowledge of appropriate work-place behavior; and the job placement services were expected to help them secure employment. A description of the implementation of these services in each site is provided in Appendix A. This section of the chapter presents the results of analyses designed to test differences in employment outcomes between participants (experimentals) and control group members.

One measure of the effectiveness of the program in meeting its goals is the proportion of participants who were able to get jobs after leaving the program, as compared to the proportion of controls who obtained employment during a comparable period. For this analysis (as well as those that follow), data were collected on experimentals for the period between program termination and the eight-month follow-up interview, and for controls, between exit date (six months after intake date) and the eight-month follow-up interview.* Overall, experimentals were more likely to have obtained employment since exit than were controls ($X^2=14.82; df=1; p<.0001$). In response to the question, "Have you worked since (exit date)," 51% of the experimentals answered yes, as compared to 41% of the controls. In all three sites, experimentals were more likely than controls to have worked at some time since exit; however, the likelihood of employment varied by site, as did the magnitude of the difference between experimentals and controls. It can be seen from the data in Table 33 that Albuquerque experimentals and controls were more likely to get jobs than members of the Miami and New York samples. Furthermore, while in all three sites a higher percentage of experimentals than controls had obtained employment, the difference was largest in the New York site. The reasons for the size of the program effect in New York are unclear. Since the job development component was implemented most effectively in Miami, one might expect that site to have the highest proportion of experimentals placed in jobs. Clearly this did not happen; Miami experimentals did only

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* Although the 8-month follow-up interview was supposed to be administered eight months after exit, in reality, researchers were not always able to contact subjects within eight months. The site researchers continued to attempt to reach subjects for 8-month follow-ups until the end of data collection; therefore, more than a year might have elapsed between exit and 8-month follow-up. Since there is no evidence of systematic variation among respondents based on the length of the "8-month" period, analyses were conducted on the data, regardless of elapsed time.

TABLE 33

Percent Of Sample Who Worked Since Exit*

	Site		
	Albuquerque	Miami	New York
Experimental	64.4%	47.2%	43.9%
(N)	(219)	(284)	(230)
Control	55.3%	41.6%	27.1%
(N)	(188)	(250)	(207)
	$X^2=3.469$ df=1 p=.06	$X^2=1.678$ df=1 ns	$X^2=13.453$ df=1 p=.0002

* The Ns represent the total number of subjects in each group who responded to the question.

slightly better than New York experimentals, and much worse than those in Albuqeruque. The very small proportion (27.1%) of New York controls who worked at all during the follow-up period suggests that the labor market in New York was less rewarding for high risk youth in general than was the market in the other two sites.

Another goal of the AYES project was to improve the ability of participants to retain jobs once they found them. Those subjects who indicated that they had worked at some time during the follow-up period were asked whether they were still working.* On this question too, experimentals were significantly more likely to respond positively; 55% of the experimentals, as compared to 46% of the controls, were working at the time of the follow-up interview (X^2=4.43; df=1; p<.05). This trend was evident in all three sites.

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* This measure of job retention is somewhat unreliable since it focuses on a single point in time -- the day of the follow-up interview. The person who worked for the previous eight months and lost his job the day before the follow-up interview is treated the same as a person who has never held a job for more than a week. A more reliable measure of job retention is the length of time (in days, weeks, or months) the person is able to hold a job. This variable is discussed below.

On measures of weekly salary for the most recent job; percent of time since exit that the person was employed; total earnings since exit; and the average job duration; the experimental group's mean was significantly higher than the control group's mean. However, as will be shown below, these data seem to reflect the greater proportion of experimentals who obtained work, and do not indicate any difference in the quality of jobs they obtained. For each of the above variables, if we examine the subsample of AYES participants who had at least one job during the follow-up period, we find no differences between experimentals and controls.

Detailed data were collected on the most recent job during the follow-up period. These data included the date the person started and terminated employment at that job; wages (hourly, weekly, biweekly, monthly, as appropriate); number of hours worked in an average day; number of days worked in an average week; type of work (occupation); and industry. The data on wages and hours were used to calculate the weekly earnings for that job; thus, for example, a participant who was paid $3.35 an hour for 20 hours per week had a weekly wage of $67.

An analysis of variance was computed on weekly earnings for the most recent job. The independent variables were site, treatment, sex, and referral source (the effects of referral source are discussed in Chapter VII). For the sample as a whole, experimentals had significantly higher weekly earnings than did controls*; there were significant differences in weekly earnings among the sites**; and there was a significant interaction between site and treatment.***

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* F(1,1372)=15.42;p=.0001

** F(2,1372)=5.22;p=.0055

*** F(2,1372)=3.28;p<.04

It can be seen from the means in Table 34 that while experimentals earned more per week in every site, the significant interaction effect reflects a substantial difference between New York experimentals and controls. It was the New York site in which the difference between experimentals and controls on percent who worked during this period was largest; therefore, it is not surprising that the difference in mean weekly earnings between experimentals (with a mean of $59.12) and controls (whose mean was $32.78) would be greatest for New York. Since a subject who did not work during the follow-up period had weekly earnings of $0, the large proportion of unemployed New York controls depressed the mean. However, if we consider only those subjects who had worked during the follow-up period, there is no significant treatment effect; the mean weekly earnings for the experimentals who had a job was $138, and the mean for the controls was $134.

TABLE 34

Mean Weekly Earnings

	Site			
	Albuquerque	Miami	New York	Total
Experimentals	$68.39	$62.37	$59.12	$63.16
(N)	(220)	(286)	(230)	(736)
Controls	52.82	56.12	32.78	47.67
(N)	(190)	(249)	(207)	(646)
Total Sample	61.18	59.46	46.65	55.92
(N)	(410)	(535)	(437)	(1382)

Detailed data were also collected on the prior job (if it fell within the follow-up period), and starting and ending dates were collected for two additional prior jobs within the period. Most AYES subjects who worked had only one job during the follow-up period, and only 3 percent had more than two jobs. These data were used to calculate the percent of the follow-up period during which the subjects were employed.

An analysis of variance (with the same independent variables as in the analysis above) was computed on the percent of the follow-up period spent working. Experimentals, with a mean of 26.2%, were employed for a significantly greater percentage of the time than were controls, whose mean was 19.2%.* Although there was a significant site effect (F(2,1372)=18.10;p=.0001), experimentals in all three sites had higher means than controls. The mean percent of time working was highest in Albuquerque (30.10), followed by Miami (22.87), and lowest in New York (16.29). Again, an examination of these data for the subsample who had at least one job reveals no significant difference between experimentals and controls, with an overall mean of 53.00 percent. Thus, even among those AYES subjects (experimentals and controls combined) who worked at some time during the follow-up period, the average subject was employed for about half the period. These youth appear to have relatively short periods of employment followed by periods of unemployment; the mean length of time an AYES youth held a job was 126 days, or approximately four months.

The final measure of employment was the total amount earned from employment since exit. Since this variable is a function of length of employment and weekly earnings on the most recent job, we would expect the results to parallel those for the other employment variables. They do; experimentals earned significantly more than controls during the follow-up period (F(1,1372)=11.64;p=.0007). The mean earnings for experimentals was $1667.70, as compared to $1183.30 for controls. Furthermore, total earnings were highest in Miami, with a mean of $1655.34, followed by Albuquerque, with a mean of $1474.40, and lowest in New York ($1148.12).* This site effect is somewhat puzzling; the means for earnings on the most recent job were approximately equal for Albuquerque and Miami (61.18 and 59.46), and the percentage of time worked during the period was higher in Albuquerque than Miami (30.10 vs. 22.87). This apparent contradiction may reflect a weakness in the computation of the total earnings variable; that is, the length of time between exit and the 8-month follow-up interview was not considered in the computation of total earnings. Therefore, the longer the period between these two interviews, the more time available to the subject to earn money. If the average length of time between interviews was longer in Miami than in Albuquerque, subjects with equal weekly wages and an equal percentage of time working would be likely to have higher total earnings in Miami. Because of this weakness, and because the information in this variable is essentially carried by the other employment variables, we do not focus on total earnings in the more complicated analyses presented below. Despite the problems with this variable, for the subsample who held at least one job during the follow-up period, there was no difference between experimentals and controls on total earnings. The mean earnings for that subsample during the follow-up period was $3,779.58.

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* F(1,1372)=15.02;p=.0001

It is clear from the data that the only employment benefit experienced by the AYES participants was the increased likelihood of getting a job. While getting a job is far from trivial, the program does not appear to have improved the quality of jobs obtained by the participants; nor did it increase the length of time they retained these jobs. It is possible that the likelihood of obtaining a job and the quality of these jobs differed among the models; these questions are explored below. It is also possible that some participants had greater benefits from the program than others (e.g., those who stayed in the program longer or those who came into the program with higher education levels). These issues will be discussed in Section C of this chapter and in Chapter VII.

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* $F(2,1372)=4.54; p=.01$

B. The Three Program Models

Having determined that the major AYES employment impact was increased likelihood of finding a job, the data were examined for variations among the three program models. Knowing that there was a treatment effect is important, but it is also important to know whether one of the models worked better than the other two, and whether there was any variation among the sites.

For each site, program model was cross-tabulated with the variable that measured whether the respondent had worked since exit. There was no significant effect for model in either Albuquerque or Miami. When controls were included as a fourth model, there was a significant effect in the New York site; experimentals were more likely to have worked than controls, but there were no differences among program models (I, II, III) in the likelihood of obtaining a job. Thus, it appears that while program participation increased the likelihood of having a job during the follow-up period, the specific model in which the subject participated did not have an effect on whether he/she obtained a job. This implies that the effect is probably due to a program factor that was present in all models. One obvious possibility would be job development services. As can be seen from Appendix A, however, the effectiveness of job development services varied by site. The job development effort was most well developed in Miami, but in Miami, the difference between the percent of experimentals who were employed and that for controls was smaller than in the other two sites. This further suggests that the AYES program effect was related to local labor market conditions.

An analysis of variance was computed on each of the major employment variables (weekly earnings on the most recent job, percent of the follow-up period spent working, and total earnings during the follow-up period). In each of these analyses, the model variable had four categories: Models I, II, and III, and the control group. These analyses indicated that model had a statistically significant effect on each of the dependent variables. (The sample included everyone interviewed at the time of the 8-month follow-up.) However, multiple comparison tests were performed and showed that while the means for the control group were statistically different from those of the models, the means for Models I, II, and III were found to be equal. Therefore, the significant model effect is really the result of the difference between the experimental group as a whole and the control group.

Moreover, when only those research subjects who had held at least one job during the follow-up period were included in the analyses, there was only one significant model effect -- total earnings. In this analysis, there was a significant model effect ($F(3,503)=3.75; p=.01$). Furthermore, the results of the comparison test on the means (Duncan Multiple Range Test) indicated that the mean total earnings of Model I participants who worked were higher than the means for Model III participants and higher than mean earnings in the control group.* These means are presented in Table 35.

TABLE 35

Total Earnings Since Exit (Working Subsample)

	Model			
	I	II	III	Control
Mean	$4676.94	$3800.96	$3449.15	$3555.41
(N)	(96)	(95)	(121)	(215)

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* While Model I participants' mean earnings were not significantly greater than the mean earnings of Model II participants, they did earn 23% more during the follow-up period.

This earnings effect cannot be explained in terms of model differences in the number of people who were employed during the follow-up period -- the models were not significantly different in that respect. Nor is the effect attributable to the differences among models in the proportion of participants who were female. Although males earned significantly more than females, the model effect is present even within gender. That is, male Model I participants earned an average of $4697 during the follow-up period, while male participants in Model II and III and male controls earned approximately $3650. Furthermore, female Model I participants, with mean total earnings of $4600, had earnings equal to the male Model I participants and higher than female participants in the other two models or female controls. The higher earnings of Model I participants also are not attributable to their having worked a higher percentage of the period than other AYES subjects. While the model effect on weekly earnings was not significant, Model I participants did earn more per week than the other subjects. The mean weekly earnings for (working) Model I participants was $147, as compared to $132 for Model II, $135 for Model III, and $134 for controls. Thus, it appears that among those subjects who got jobs, the Model I participants tended to get jobs that paid more.

C. Theories of Labor Market Structure

The results presented above indicate that the only appreciable effect of the AYES program on employment was that it increased the probability of post-program employment for participants. This, in turn, resulted in significant program effects on weekly wages, percent of time employed, and total earnings. When characteristics of the jobs were examined for those members of the experimental and control group who worked, the results indicated that the jobs obtained by experimentals were no better or longer lasting than those obtained by controls.

As previously indicated, the AYES program rests essentially on a human capital theory of labor supply. We felt it useful therefore, to derive from that theory some hypotheses that could be tested on our data base and thus assess the extent to which the theory helps us to understand the effects, or non-effects, of the program. We also derived some contending hypotheses from labor market segmentation theory and tested them to see if that theory is any more helpful in understanding the impact of the program.

In this section, then, the human capital hypotheses and the results of our tests are presented and discussed before according similar consideration to the suggestions of segmented labor market theory.

1. Human Capital Theory

Human capital theory is best articulated by Gary Becker in Human Capital (1964). This theory is a development of the neo-classical competition model, which allows for examination of and policy development for dealing with poverty and racism. As a neo-classical model, it views the economy as tending towards equilibrium and sees effort as getting fair rewards. This state of affairs develops from the choices of individuals about where to invest their money, whom to hire, where to work, and how to spend their time. The decision that is at the heart of human capital theory is whether to spend time and resources to obtain income in the present or, instead, to use them in obtaining skills and knowledge that will increase future income.

Thus, education, knowledge, and skills are treated as investment. Like any other investment, if not misdirected, such investment in "human capital" will earn a fair return, since it is assumed, employers will pay for the marginal increment in human capital an amount equal to the marginal increase in value produced.

The most important claim of human capital theory for the purposes of the AYES research is that education and training increase earnings and employment possibilities. Although some, e.g., Blaug (1976), would question the claim that human capital is responsible for income differences (rather than family background or other prior causes), employment training programs such as AYES are based on this assumption. The literature on training programs, while not conclusive, offers experimental evidence in which random assignment to experimental and control groups allows the effects of training (as an increase in human capital) to be partially isolated from these prior variables. In a review of a number of experimental (or comparison group) studies of youth and adult training programs, Borus (1980) concluded that classroom, on-the-job training, and work-experience programs "appear to yield benefits sufficient to justify the programs if the first year benefits continue relatively intact for five or more years. The evidence of the continuation of benefits, however, is not clear"(p.35). He found that participants' gains for the first year were in the hundreds of dollars.

Thus, AYES participants should increase their human capital and, therefore, increase earnings relative to the control group. In addition, AYES training should give them new skills and teach them proper job behavior. From a human capital viewpoint the new skills mean that employers who hired AYES trainees would spend less on training costs than would have been true in the absence of AYES and, similarly, that losses due to wasting time and resources on new hires whose bad behavior leads to their firing can be reduced. As a result of these lowered training and screening costs, more jobs should be open to AYES experimentals and their unemployment rate should decrease relative to the control group.

Human capital theory also holds that differences in returns to investment in human capital among occupations, industries, races, and genders tend to be short-lived deviations from an average rate of return. The differences are eliminated as producers shift their investments or their hiring to take advantage of these differences in return. Thus, if women get paid less than men of equal skills, employers will increase their hiring of women to take advantage of this cheap labor force and women will invest less in these particular skills. This, in turn, will increase women's pay (due to increased demand and decreased supply) and decrease men's, such that equilibrium will be restored with each gender getting the same return to education.

The evidence on variations of rates of return to educational investment is mixed. Kalleberg et al. (1981) found that men and women get the same (positive) return to education. Zucker and Rosenstein (1981) found that the contribution of education to earnings is equal across clusters of industries. However, Beck et al. (1980) found that dollar returns to human capital are greater for whites than nonwhites, men than women, and workers in core industries than workers in peripheral industries. McGahey (1982) found a higher rate of return to education in the primary labor market than in the secondary. Finally, Taylor et al. (1981) found variations in rates of return to sub-college education by sex, race, and industrial sector. The data collected in the AYES study can be used to test these hypotheses for high risk youth.

Neo-classical theory, and Becker (1968) in particular, have considered the economics of criminal behavior and its relationship to human capital. In this approach, the individual is assumed to make rational choices about whether to invest time and resources in legal or illegal income-producing behavior. Thus, higher returns to legal work should make illegal work less attractive, and income-producing criminal activity should be negatively associated with earnings and with the amount of time spent in legal work.*

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* Block and Heineke (1975), however, used neo-classical logic to show that increases in returns to legal (or illegal) activity need not result in a decrease in the other activity. Ehrlich (1979) reviewed the literature and found that it supports the neo-classical theory of crime. Thompson _et_ _al_. (1981, ch.2) were more critical of this approach.

McGahey (1982:86f) asserted that time spent on crime and time incarcerated reduce legal work payoffs and opportunities, both by reducing the training that takes place in legal work (which would increase human capital), and from the stigma of a criminal record. This, then, implies that prior crime (and, particularly, prior arrests) should reduce human capital and limit available jobs and, thus, that prior crime should be associated with lower earnings and less work at a later date. However, McGahey's (1982) own results for a sample of male arrestees cast doubt on this hypothesis.

For the AYES sample, human capital theory leads to the following hypotheses:

a. There should be a positive association of human capital variables such as being a program participant rather than a control and education level or STEP score (a measure of reading ability) with outcome measures such as weekly earnings and percent of time employed.

b. These returns to human capital should be equal across race* and gender.

c. Arrests prior to AYES participation should be negatively associated with later earnings and time employed.

The method used to test the hypotheses derived from human capital theory (and used to test other hypotheses, below) is ordinary least squares regression analysis for continuous dependent variables and logistic regression analysis for dichotomous dependent variables. For both types of analysis, categorical independent variables (e.g., site and model) were dummy coded to allow their use in the equations. The discussion of these analyses focuses on the interpretation of the results, and tables of the relevant statistics are provided. For many of these analyses, as well as others presented throughout this report, statistical significance may be achieved as the result of a very small effect. This problem is especially prevalent in those analyses computed on the entire sample for whom 8-month follow-up data were available (N>1300). Therefore, both statistical significance and absolute magnitude (effect size) are considered in assessing the relevance of these effects and their support for the theories. (To assist the reader in reaching his/her own conclusions, the tables contain both regression coefficients and significance levels.)

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* Although most tests of the theory would compare returns to human capital for whites and non-whites, the composition of the AYES sample does not allow for such comparisons, because except in Albuquerue, there were virtually no whites in the sample. Therefore, we don't report results of tests on race.

In each analysis, age at intake, sex, total earnings in the year prior to intake, marital status (respondents who were married and living with their spouse at the time of the 8-month follow-up are considered married; all others are considered not married), and site were considered covariates (control variables). In the tests of human capital theory, highest grade in school completed (prior to intake) and STEP score are considered pre-program human capital variables; number of arrests prior to intake is used to represent prior crime; and treatment (experimental or control) is used to measure the human capital impact of the AYES program.

The first human capital hypothesis to be tested is the effect of the program and other human capital variables on employment outcomes; these outcomes were getting a job, weekly earnings for the most recent job during the follow-up period, and percent of the follow-up period the respondent was employed. The analyses presented in section A of this chapter indicated that experimentals were more likely to have been employed during the follow-up period than were controls. (This conclusion was based on a simple cross-tabulation.) To test the human capital hypothesis, a logistic regression was computed on whether the subject had a job during the follow-up period; the covariates and human capital

TABLE 36

Logistic Regression Analysis
Dependent Variable: Did Subject Have A Job Between Exit and 8-Month Follow-up

Independent Variable	Beta	Standard Error	Chi-Square	p	D
Intercept	-1.068	.987	1.17	.2793	-
Age at Intake	.008	.050	.03	.8687	.000
Gender (M=1, F=2)	- .960	.160	35.82	<.0001	.037
Total Earnings, Year Before Intake (All Subjects) (In Thousands of Dollars)	.080	.045	3.21	.0731	.003
Marital Status (Living With Spouse=1)	- .084	.255	0.11	.7408	.000
School Years Completed	.121	.066	3.40	.0652	.004
STEP Score	.016	.015	1.21	.2707	.001
# Arrests, Pre-Intake	- .018	.045	0.17	.6812	.000
Albuquerque (Dummy)	.951	.183	26.93	<.0001	.028
Miami (Dummy)	.275	.179	2.37	.1238	.003
Segment (All Subjects Employed During Year Before Intake)	- .113	.157	0.60	.4394	.001
Model 1 (Dummy)	.221	.194	1.29	.2552	.001
Model 2 (Dummy)	.404	.193	4.37	.0366	.005
Model 3 (Dummy)	.865	.193	20.11	<.0001	.021

Equation Statistics:

$N = 954$

Equation $D=.096$

-2 Log Likelihood = 1221.96

Model $X^2=100.15$ $df=13$ $p(X^2)<.0001$

variables were those described above. The results of this analysis are presented in Table 36, and they provide mixed results with respect to the human capital hypothesis.

The best predictors of employment were gender, site, and program model. Males were more likely than females to have been employed during the follow-up period. As was indicated by the cross-tabulations (in Table 33, above), the subjects who lived in Albuquerque, regardless of treatment, were more likely to be employed than those in the other two sites. The measure of treatment used in the logistic analysis was program model (dummy variables). The positive betas for all three model variables indicate that experimentals were more likely to be employed than were controls; however, the effects were significant only for Models II and III.*

The effect of program participation on post-program employment seems to support the human capital hypothesis. However, the hypothesis is challenged by the nonsignificance of two other human capital variables -- schooling completed and STEP score -- and by the significant effects of two variables -- site and gender -- which clearly are not human capital variables.

Knowing that experimentals were more likely to secure employment than were controls, it is important to determine whether the program and other human capital variables affected the quality of that job (as measured by weekly earnings) or the percentage of time the respondents spent employed. A multiple regression analysis was computed on weekly earnings for the most recent job using the standard set of variables. When all subjects (including those who did not work at all during the follow-up period) are included in the analysis, all the predictors together explain a total of only 8% of the variance in weekly earnings (See Table 37). The best predictor of weekly earnings is gender, which explains 2.4% of the variance (after controlling for age). Males earn significantly higher wages than females, which would be expected, if for no other reason than they are more likely to be employed. The only other variables which explain more than a minuscule amount of variance are site and treatment, each explaining approximately 1% of the variance. These effects are also attributable to the differences in probability of having a job. When only those subjects who worked are included in the analysis, also presented in Table 37, 12% of the variance in earnings is explained. Among those who worked, gender accounts for 3.3% of the variance in earnings; again, males earn significantly more than females. (The average weekly pay for working males (N=385) was $142.91, as compared to the mean for females (N=165) of $122.14.) There was a weak effect (explaining 1.4% of the variance) for marital status, with married AYES subjects earning more than unmarried subjects. This effect for marital status is significant even when controlling for age and gender. Among working subjects, wages were lowest in Albuquerque; site accounts for 5.6% of the variance. Absent from the list of meaningful effects on earnings are the human capital variables. Once the control variables are in the equation, the subjects with relatively more years in school do no better than those with fewer years of education. In addition, being in the AYES program has no impact on weekly wages of working subjects.

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* This more powerful form of analysis also suggests that the subjects' model did have an effect on the likelihood of employment during the follow-up period. As previously indicated, no such effect was discernable in the cross tabular analysis. The different conclusions reflect the fact that the logistic regression was applied to the entire eight month sample (N>1300) and is a more powerful statistical technique. Statistical significance is not difficult to achieve with so large an N, but the size of the effect seems quite small. Therefore, the effect of program model on post-program employment may not be very meaningful.

Similar results were obtained for the other dependent variables used to test this hypothesis. None of the human capital variables had appreciable effects on the percent of the follow-up period spent in employment. For the sample as a whole, the predictors explain 9% of the variance, with the best predictors being

TABLE 37

Ordinary Least Squares Analyses of Economic Outcomes

	Weekly Earnings of Most Recent Job					
	Subjects Who Worked			All Subjects		
Independent Variable	b	p*	Beta	b	p*	Beta
Intercept	89.55	.0035	-	-0.92	.9738	-
Age At Intake	2.26	.1378	.07	2.43	.0857	.05
Gender (M=1, F=2)	-19.95	<.0001	-.17	-29.26	<.0001	-.19
Total Earnings, Pre-Intake Year (All Subjects) (In Thousands of Dollars)	0.45	.7312	.01	3.24	.0203	.06
Marital Status (Living With Spouse=1)	21.46	.0033	.12	21.96	.0035	.08
School Years Completed	2.69	.1655	.06	4.33	.0129	.07
STEP Score	.85	.0516	.09	1.01	.0111	.08
# Arrests, Pre-Intake	1.55	.3317	.04	-0.85	.5359	-.02
Albuquerque (Dummy)	-25.70	<.0001	-.24	12.73	.0168	.08
Miami (Dummy)	3.38	.5737	.03	17.22	.0009	.11
Treatment (0=Experimental, 1=Control)	-3.36	.4398	-.03	-14.39	.0003	-.10
Equation Statistics:						
N			544	1335		
Model df			10	10		
Error df			533	1324		
F			7.122	10.945		
p (F)			<.0001	<.0001		
R^2			.1179	.0764		
Adjusted R^2			.1013	.0694		

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* Probability computed from t-test on regression coefficient.

gender, site, and treatment. The results of the analysis on the working subsample imply that the treatment effect reflects the greater likelihood of experimentals to find employment. Both analyses are presented in Table 38, and it can be seen from the table that the effect of treatment disappears in the subsample of working subjects. For those who worked, 8% of the variance is explained, most of it by age and marital status. The correlation between age and percent of time working (for the subsample) was r=.15; older subjects tended to spend a higher percentage of the follow-up period employed. Even after controlling for age and gender, married subjects tended to spend more of the period in employment (explaining about 2.4% of the variance).

Neither the analyses on weekly earnings nor the analyses on percent of time working supports the hypothesis that human capital variables should be positively associated with employment outcomes. They suggest, rather, that to the extent that we can explain the variance at all, employment outcomes are best predicted by gender, marital status, age, and site. All of these are considered covariates for this analysis, and are not addressed by the theory.

The second human capital hypothesis was that the returns to human capital should be equal across gender. (Although the human capital variables, with the exception of treatment, showed virtually no relationship to employment outcomes, it is possible that some segments of the sample would benefit from increases in human capital.) To test this hypothesis, separate regression equations were run for males and females. Regression coefficients for the human capital variables were tested to see whether they were significantly different in the separate equations. For example, a Z-test was done to test the difference between the regression coefficient obtained for the analysis of males on years of education and that obtained for females. There were no instances in which the regression coefficient for males was significantly different from that for females. Thus,

TABLE 38

Ordinary Least Squares Analyses of Economic Outcomes

	Percent of Exit to 8-Month Follow-Up Period Working					
	Subjects Who Worked			All Subjects		
Independent Variable	b	p*	Beta	b	p*	Beta
Intercept	-15.60	.4291	-	-17.03	.1809	-
Age At Intake	3.11	.0014	.14	1.71	.0080	.08
Gender (M=1, F=2)	- 4.11	.1953	-.06	-10.03	<.0001	-.14
Total Earnings, Pre-Intake Year (All Subjects) (In Thousands of Dollars)	1.64	.0514	.08	2.09	.0010	.09
Marital Status (Living With Spouse=1)	15.65	.0012	.13	9.65	.0048	.08
School Years Completed	0.93	.4571	.03	1.77	.0252	.07
STEP Score	0.34	.2230	.06	0.38	.0345	.06
# Arrests, Pre-Intake	-0.99	.2950	-.04	-0.52	.4074	-.02
Albuquerque (Dummy)	5.94	.1083	.09	12.35	<.0001	.16
Miami (Dummy)	9.39	.0147	.14	7.83	.0009	.11
Treatment (0=Experimental, 1=Control)	-4.91	.0755	-.07	- 7.07	<.0001	-.10

Equation Statistics:

N	577	1335
Model df	10	10
Error df	566	1324
F	4.771	13.031
p (F)	<.0001	<.0001
R^2	.0777	.0896
Adjusted R^2	.0614	.0827

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* Probability computed from t-test on regression equation.

there is some support for this hypothesis in the AYES data. The hypothesis of equal returns to human capital is a very narrow one, however; it does not address the substantial differences between males and females that are clear from the data. AYES males are more likely than females to be employed, and once employed, are likely to earn higher wages. All that can be concluded from the test of this hypothesis is that, given their unequal starting points, males do not appear to get any more from the AYES program than do females.

The final human capital hypothesis to be tested is that arrests prior to AYES intake should be negatively associated with later earnings and time employed. There is no evidence from the AYES data to support this hypothesis. After controlling for age, sex, education, marital status, and prior earnings, there was no relationship between number of arrests prior to intake and weekly wages; nor was there any relationship with percent of time working. This was true for both the sample as a whole and the working subsample, and can be seen from the results in Tables 37 and 38. The correlations of these variables with number of arrests were also not significantly different from zero; the highest was $r=.06$. Furthermore, number of arrests had no effect on the probability of employment during the follow-up period.

Taken together, the tests of these hypotheses provide little support for the human capital model. The only meaningful effect of a human capital variable was that experimentals were more likely to get a job during the follow-up period than were controls. Furthermore, very little of the variation in employment outcomes is explained with all of the variables (the highest R^2 is .12), and most of that is explained by site and gender, neither of which is considered a human capital variable. Most of the differences in outcomes among AYES subjects are unexplained. Even the largest treatment effect, the increased probability of experimentals to get a job was rather weak -- 51% of the experimentals worked, as compared to 41% of the controls.

Perhaps the human capital model fails to explain employment outcomes because it is not an accurate description of the structure of the labor market. Segmentation theory offers an alternative model which leads to different hypotheses about the effects of the AYES program on employment outcomes.

2. Segmented Labor Market Theory

The theory of labor market segmentation has been developed by such institutionalist and Marxist critics of the neo-classical approach as Doeringer and Piore (1971, 1974), Berger and Piore (1981), Edwards (1979), Edwards et al. (1975), Gordon (1971, 1977), Gordon et al. (1982), Harrison (1977, 1979), Osterman (1975, 1977, 1980), and Piore (1973, 1975, 1979). It is based on an observationally-grounded hypothesis and attempts by the above authors to give this hypothesis a theoretical basis. The hypothesis holds that the labor market is divided into two different segments with the following characteristics: the primary segment is composed of jobs with career structures (either job-ladders within a firm or professional or craft career patterns external to any given firm), relatively high job security, better pay and working conditions, and procedural rationality in personnel decisions. Secondary jobs lack career structures, have high labor turnover and little chance of advancement, low pay, poor working conditions, and supervision that is often arbitrary. The boundaries between segments initially were thought to be rather rigid, but a number of studies (Andrisani, 1973; Buchele, 1976; Lowell, 1973; Rosenberg, 1975) have shown there to be more mobility between segments than was originally thought. Nonetheless, mobility between segments is still seen as difficult. In particular, workers in the secondary labor market tend to be trapped there (and, indeed, secondary work experience is thought to stigmatize and thus make it harder for them to get a primary job), and small differences in education are thought to have no value in helping workers to escape into the primary market. Andrisani (1973), for instance, finds that human capital has no marginal impact on mobility between secondary and primary segments and concludes that manpower programs based on human capital investment are ineffective.

As segmentationists see it, the primary labor market operates according to the neo-classical model. Thus, the hypotheses given above for the human capital approach should apply within a primary labor market subsample.

Within the secondary segment, on the other hand, segmentationists see differences among workers as having little or no impact on their earnings or job quality. Thus, they see human capital and prior criminal involvement as unassociated with labor outcomes (McGahey, 1982).

In the secondary labor market, it is hypothesized that many people move among employment, welfare, training programs, and criminal activity (Harrison, 1979) in an effort to meet their material needs. None of these forms of support is seen as much of a barrier to moving to another. As a result, secondary workers engage in more criminal activity than primary workers, and this should not be affected by participation in training programs such as AYES.

Segmentationist theory would predict the following for AYES:

a. Within the primary segment, the relationships expected by human capital theorists should hold; however, within the secondary segment, human capital and prior crime should not be associated with labor outcomes (such as weekly earnings or percent of time employed).

b. Labor outcomes such as earnings and time employed should be more positive in the primary segment than in the secondary. Criminal activity (as measured, for example, by arrests) should be higher in the secondary segment.

c. There are difficulties in classifying workers, rather than jobs, as primary or secondary. However, it seems reasonable that all or almost all the individuals studied in this project tend to be in the secondary labor market since almost all are minority, all are poor, and all found it reasonable to apply for the AYES program. Indeed, Harrison (1977) views training programs as a constituent part of the secondary labor market. In summary, then, we can formulate the following hypothesis on the premise that the AYES sample is a secondary sample. Since, as discussed above, "human capital has no marginal impact on mobility between secondary and primary segments," human capital should not be associated with the segment of AYES subjects' jobs.

d. Segmentation theory has not sufficiently come to terms with the existence of inter-segment mobility to develop much theory about it. However, the theory does argue that racism and sexism are important determinants of who gets confined to the secondary segment, which leads to the hypothesis that being male should be associated with getting a primary job.*

e. About a third of participants' most recent jobs at intake were primary jobs. This suggests that the Harrison-based approach discussed in (c) might be wrong and, thus, that participants might include youth with attachments to the primary segment as well as to the secondary. If so, then segmentationists would expect such attachments to affect participants' later jobs. This implies that there should be a positive association between segment of pre-intake job and segment of the most recent job during the period.

The methods used to test the hypotheses derived from segmention theory were the same as those used to test human capital theory. It was necessary, however, to define a variable which indicated the segment of the respondents' most recent job in the post-program period.** The categorization was done using the method derived by Gordon (1982) which uses occupation and industry of the job;

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* The AYES sample is virtually all minority. Thus, we cannot test the hypothesis about minority/white differences in job segments.

** The most recent job was chosen for this variable because the great majority (66% - 83%, depending on site) of those who worked had only one job.

this resulted in 32.1% of the working respondents' most recent jobs being classified as primary (representing about 15% of the sample interviewed eight months after exit).

The first segmentation hypothesis to be tested is that within the primary, but not the secondary segments, human capital theory should be supported. To test this hypothesis, the regression analyses described above were run separately for those subjects whose most recent job was in the primary segment and those whose most recent job was secondary. The regressions on weekly earnings on the most recent job did not support the hypothesis. As can be seen from the results presented in Table 39, the human capital variables (years of school completed, STEP, and being an AYES participant) have very little predictive power. For the respondents whose most recent jobs were primary, 15% of the total variance in earnings was explained; however, 5% was attributable to gender and 4% to site. Only 1% of the variance in earnings was accounted for by grade in school and STEP combined, and 2% by program model. Thus, contrary to what segmentation theory would predict, human capital variables were not related to earnings in the primary segment. Nor were they related to earnings in the secondary segment. For those respondents whose most recent job was in the secondary segment, 14% of the variance in earnings was explained. Again, the human capital variables accounted for virtually none of the variance,* while sex explains 3% and site explains 6% of the variance in weekly earnings of these subjects. The results of the analysis on the percent of time that employed subjects worked provided no support for the hypothesis. Human capital variables accounted for virtually none of the variance in either the primary or the secondary segment. While 17% of the variance in percent of time working was explained for workers in primary jobs, this was

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* Although the regression coefficient for the Model I dummy variable indicated that is was significantly different from zero, the three dummy variables together account for 2% of the variance.

TABLE 39

Regression Coefficients of Human Capital and Prior Arrests by Segment*

Weekly Earnings of Most Recent Job Since Exit

	Subjects Who Worked					
	Primary			Secondary		
	b	p	Beta	b	p	Beta
Age	1.67	.4707	.06	2.58	.1999	.07
School Years Completed	3.53	.2492	.09	1.36	.5876	.03
STEP Score	1.51	.0463	.17	0.66	.2280	.07
Albuquerque (Dummy)	-17.77	.0573	-.19	-30.94	.0001	-.27
Miami (Dummy)	5.32	.5766	.06	3.22	.6738	.03
Model 1 (Dummy)	4.56	.6335	.04	18.71	.0171	.13
Model 2 (Dummy)	-7.28	.4435	-.07	-1.10	.8907	-.01
Model 3 (Dummy)	11.95	.1718	.11	-5.41	.4512	-.04
# Arrests, Pre-Intake	0.32	.9110	.01	1.35	.4945	-.04
N	180			364		
p(F)	.0042			<.0001		
R^2	.1514			.1433		
Adjusted R^2	.0904			.1140		

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* The other variables have been omitted from the table, but were included in the analysis. See Table 38 for the complete list of variables.

explained by age (3%), marital status (6%), and site (3%). Program model accounted for 2% of the variance, with Model I subjects tending to work more than those in Models II and III and in the control group. Only 6% of the variance in percent of the follow-up period spent in employment was explained for those in secondary jobs, and none of it was due to human capital variables (see Table 40).

TABLE 40

Regression Coefficients of Human Capital and Prior Arrests by Segment*

	Percent of Exit to 8-Month Follow-Up Period Working					
	Subjects Who Worked					
	Primary			Secondary		
	b	p	Beta	b	p	Beta
Age	4.18	.0130	.19	2.59	.0359	.12
School Years Completed	.59	.7907	.02	1.10	.4800	.04
STEP Score	.65	.2378	.10	.30	.3578	.05
Albuquerque (Dummy)	6.13	.3470	.09	5.96	.1890	.09
Miami (Dummy)	17.02	.0131	.25	6.00	.2021	.09
Model 1 (Dummy)	14.73	.0318	.17	6.89	.1454	.08
Model 2 (Dummy)	.85	.8994	.01	4.32	.3719	.05
Model 3 (Dummy)	6.88	.2722	.09	1.48	.7365	.02
# Arrests, Pre-Intake	-.04	.9857	.00	-1.04	.3425	-.05
N	180			397		
p(F)	.0009			.0249		
R^2	.1720			.0571		
Adjusted R^2	.1126			.0276		

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* The other variables have been omitted from the table, but are included in the analysis.

Thus, the first segmentation hypothesis was not supported by the AYES data.

The second hypothesis generated by segmentation theory was that economic outcomes would be better for those AYES subjects who obtained jobs in the primary segment than for those who had secondary segment jobs. To test this hypothesis, an analysis of variance was computed on each of the following variables: weekly earnings on the most recent job, total earnings during the follow-up period, percent of the follow-up period employed, and average number of days worked on a job. For each analysis, the segment of the most recent job was the first independent variable, followed by site, model, sex, referral source, and their interactions with segment. The means on each of the dependent variables for the two segments are presented in Table 41. The difference between segments was significant for total earnings and percent of the period employed, and was in the predicted direction. In each case, however, segment accounts for only 1% of the variance. Thus, this hypothesis receives weak support from the working subsample of AYES subjects.

TABLE 41

Mean Values At 8-Month Follow-Up For Those Who Worked After Exit, By Segment
(Number Of Cases In Parentheses)

	Most Recent Job Prior To 8-Month Follow-Up		Statistics On Hypothesis That Segments Are Equal	
	Primary	Secondary	Statistic	p
Weekly Earnings	$ 137 (185)	$ 136 (379)	F(1,542)=0.15	NS
Total Earnings, Exit To 8-Month	$4,230 (167)	$3,585 (356)	F(1,501)=5.04	.0252
Percent of Period Working	57.3% (185)	51.1% (407)	F(1,570)=4.47	.0348
Mean Job Duration	137 Days (185)	121 Days (411)	F(1,574)=3.53	NS

To determine whether human capital variables were related to the segment of AYES subjects' jobs, a logistic regression analysis was computed with segment of the most recent job as the dependent variable. It is clear from the results of this analysis (in Table 42) that none of the human capital variables (highest grade completed, STEP score, and program model) is significantly related to the

TABLE 42

Logistic Regression On Segment Of Most Recent Job

Independent Variable	Beta	S.E.**	X^2	P	D
	Gordon Segment* Subjects With Job)				
Intercept	3.03	1.56	3.78	.0519	-
Age At Intake	.03	.08	.17	.6824	.000
Gender (M=1,F=2)	-.90	.24	13.60	.0002	.030
Total Earnings, Year Before Intake (All Subjects) (Thousands of Dollars)	.05	.06	.52	.4713	.001
Marital Status (Living With Spouse=1)	-.65	.36	3.35	.0671	.008
School Years	-.13	.10	1.69	.1942	.004
STEP Score	-.04	.02	2.13	.1446	.005
# Arrests, Pre-Intake	.04	.08	.22	.6376	.001
Albuquerque (Dummy)	.39	.28	1.92	.1658	.004
Miami (Dummy)	.06	.30	.04	.8496	.000
Model I (Dummy)	-.27	.30	.84	.3605	.002
Model 2 (Dummy)	-.24	.29	.67	.4132	.002
Model 3 (Dummy)	.19	.28	.48	.4882	.001
Segment Of Most Recent Job Prior To Intake (Subjects Who Worked During Year Before Intake)	.01	.22	.00	.9599	.000

Equation Statistics:

Mean Value of Dependent Variable	.678
N	457
-2 Log Likelihood	539.19
Chi-Square	34.91
Degrees of Freedom	13
P	.0009
D	.073

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* Gordon Segment is coded 0=Primary, 1=Secondary

** S.E.=Standard error of Beta

segment of the most recent job. Thus, the results support the hypothesis that human capital is unrelated to job segment. (It should be noted, however, that since this hypothesis was stated as the null, there may be alternative explanations for this finding, more closely related to methodology than to the reality of the labor market.)

The same analysis was used to test the hypothesis that males would be more likely to get primary jobs than would female AYES subjects. The only significant predictor in the equation was gender; however, the results were in the direction opposite to that predicted. Among the AYES subjects who worked during the follow-up period, females were significantly more likely to have primary jobs than were males. This result can probably be attributed to the likelihood of those females who worked having clerical jobs, which are considered primary. The classification of clerical workers as primary may be a weakness of Gordon's classificaton scheme.

Finally, segmentation theory predicts a positive relationship between the segment of the job held just prior to AYES intake and the most recent job during the follow-up period. To test this hypothesis, segment of the most recent job prior to intake was included in the logistic regression analysis (presented in Table 42). This analysis indicated that there was no relationship between these two variables. Of the 1576 AYES subjects who worked in the year prior to intake, 34% held primary segment jobs, and of the 626 who worked subsequent to exit, 32% had primary jobs. The analysis suggests considerable movement between segments; a person who had a primary job prior to AYES was no more nor less likely than one who held a secondary job to have a primary job during the follow-up period.*

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* Due to constraints of data analysis, only the 457 subjects who were employed during both periods could be included in the logistic analysis. Therefore, the results of the analysis should be interpreted cautiously and may not apply to the entire sample.

The results of the tests of the five segmentation hypotheses, on the whole, did not support the theory. Segmentation theory correctly predicted that people in primary jobs worked a higher proportion of the time than people in secondary jobs and was also correct in predicting that human capital is not associated with segment for AYES subjects. However, it incorrectly predicted that economic outcomes would be related to human capital in the primary segment but not in the secondary; that earnings would be higher in the primary than the secondary segment; and that segment of the most recent pre-intake job would be positively associated with segment of the most recent job during the follow-up period. It also predicted that men would be more likely to be in the primary segment than women -- the reverse of what proved to be the case. Finally, a higher proportion of AYES subjects had primary jobs than segmentation theory would seem to imply.

These findings pose a serious challenge to the value of either human capital or segmentation theory as an explanation of the labor market facing impoverished, high-risk youth. Simply put, the equations explain very little of the variation in the dependent variables. If we consider the equations presented in the tables, we see no cases in which unadjusted R^2 is greater than 0.17, or in which the model D in the logistic regressions (which is similar to R^2 as a measure of how much variation is explained by the total equation) is as high as 0.10. This means that the great bulk of the difference in outcomes among individuals is unexplained by the models. Furthermore, much of what is explained by these equations is explained by the control variables -- particularly by the site dummies -- rather than by human capital or segment. As one example, Table 37 presents an equation in which 7.64% of the variance in weekly earnings (for all subjects) is explained. If we consider the human capital variables -- school years (significant with $p=.0129$), STEP ($p=.0111$), and Treatment ($p=.0003$) -- we find that together they explain only about 2 percent of the total variance in earnings.

Even in the case of the most important economic finding of the study -- that experimentals are more likely to have a job at some time in the post-exit period than are controls -- relatively little of the variation is explained. Slightly over one half of the experimentals worked during this period, but so did two-fifths of the controls. Thus, the multivariate logistic equation presented in Table 36 explained only 10% of the likelihood (D=.096), and the site dummies and gender, not human capital, were the main explanatory variables. This indicates that, once again, the theory explained little of the variation in finding jobs.

CHAPTER VI: CRIMINAL JUSTICE SYSTEM OUTCOMES

One of the goals of the AYES project was to reduce subsequent involvement with the criminal and juvenile justice system. This reduction was expected to be the result, in part, of increased employment of program participants. Since as discussed in Chapter V, the effects of AYES on post-program employment were weak, we would not expect the program to have had a strong effect on post-program arrests. Nonetheless, that effect is examined in this chapter.

Another reason to expect program effects on crime (independent of employment), is derived from the economic model of crime. According to this model, individuals divide their time between legal and illegal activities so as to maximize their gains (Thompson et al., 1981). Thus, time spent in the AYES program might be thought of as decreasing the amount of time available for illegal activities. This concept leads to two predictions: (1) experimentals should have fewer "in-program" arrests than controls and (2) among experimentals, time spent in the program should be negatively correlated with in-program arrests. The results of analyses to test the first of these hypotheses are discussed below and results of the second are discussed in Chapter VII.

Finally, other research has suggested that the amount of time spent at legitimate employment, whatever the source of that employment, is related to reduced levels of arrests. This chapter examines that hypothesis by combining the experimental and control samples and testing for a relationship between post-program arrests and the amount of post-program employment.

The presentation of analyses in this chapter follows the same structure as that in Chapter V. Section A focuses on differences between experimentals and controls on arrests and convictions subsequent to AYES intake. Section B is concerned with differences among the three program models on such variables. The analyses presented in Section C go beyond the experimental design. Possible covariates, such as age, gender, race, prior criminal history, and referral source, are included in the analysis to determine whether they predict subsequent criminal justice system involvement, and to determine whether the program is effective for any of these subgroups of AYES participants. In addition, this section contains the results of the analyses on employment and crime relationships; employment variables such as percent of follow-up period employed and weekly earnings on the most recent job were included in the analyses to determine their relationships to arrests during the follow-up period.

The data in all of the above-mentioned analyses were computed using information collected from official criminal and juvenile justice system records. As discussed in Chapter III, we initially hoped to use self-report data on criminal activity, but these proved to be unreliable.* The data collection procedures for the official record data are described in Chapter III. Analyses reported in this chapter focus on "post-intake" arrests and convictions; these variables were computed from information about the 14 months subsequent to intake. A period of 14 months was used to provide a uniform length of follow-up for all subjects, and to coincide with the six months of program participation and eight months of follow-up.

A. AYES Program Effects: Differences Between Experimentals and Controls

The total number of arrests in the fourteen months subsequent to AYES intake was counted for each AYES research subject. (Similar variables were constructed for the "in-program" and "post-program" portions of this period, as described below.) An analysis of variance was computed on total number of arrests subsequent to intake to determine whether there was a program effect on arrests. The results of this analysis provided no indication that the AYES program had an effect on the number of times an individual was arrested subsequent to intake. The mean number of post-intake arrests for experimentals in all sites was .42 and the mean for controls was .46. These are not significantly different; nor were there significant differences between experimentals and controls in any of the sites. There were significant differences in arrest rates among the three sites,* paralleling the results on pre-intake arrests. Miami AYES subjects had the highest mean number of arrests (.58), followed by New York (.42), and Albuquerque (.29). The distribution of number of arrests by site is presented in Table 43.

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* The AYES experience with self-reported illegal activities is not unique. Similar problems were reported in the evaluation of the Supported Work projects (Maynard, 1980) and in the evaluation of the Court Employment Project diversion program (Baker and Sadd, 1979).

TABLE 43

Number of Post-Intake Arrests by Site

	Albuquerque	Miami	New York
0	78.6%	70.6%	74.9%
1	15.2	15.2	14.2
2	4.9	6.7	7.3
3	1.0	4.2	1.9
4	0.3	1.6	0.7
5+	-	1.7	0.8
(N)	(611)	(759)	(830)

Typically in program evaluations, experimentals and controls are compared on number of in-program arrests and on number of post-program arrests. Such comparisons are problematic for the AYES research: each participant in AYES was entitled to a maximum of six months of program services. However, many participants either dropped out, left the program for a job, or received disciplinary terminations prior to completing the program. As a result, the in-program period for experimentals ranges in length from one day to 26 weeks. For controls, who received no AYES program services, the in-program period was defined as six months from the research intake date. Therefore, the average in-program period for experimentals is substantially shorter than that for controls. This renders interpretation of differences between experimentals and controls on arrest rates difficult. Similarly, the length of the post-program period for controls is a uniform eight months, while for experimentals, it varies from fourteen months (for those who dropped out after the first day) to eight months for those who completed the program.

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* F(2,2155)=16.32; p<.0001

Despite the problems discussed above, variables describing the number of in-program arrests and the number of post-program arrests were created for all AYES research subjects. The more meaningful analyses on these variables were limited to experimentals and are described below in Section B, as well as in Chapter VII. In addition, cross-tabulations with treatment were computed on each of these variables, by site. Because the number of arrests during these relatively short periods tended to be quite small, the variables were recoded to indicate whether or not the subject was arrested during the period.

The analyses indicated no significant relationship between treatment and the likelihood of arrest during the in-program period for Albuquerque subjects; 11% of the experimentals and 11% of the controls were arrested during this period. As discussed in Chapter III and indicated above, the arrest rate in Albuquerque was lower than that in either of the other two sites. Thus, it appears that the likelihood of arrests for the Albuquerque sample tended to be low, regardless of AYES participation. In Miami and New York, however, there were significant differences between the experimental and control groups on likelihood of arrest during the in-program period. In Miami, 9% of the experimentals and 16% of the controls were arrested during this period*; in New York, 7% of the experimentals and 13% of the controls were arrested during the in-program period.** These results could be interpreted as support for the hypothesis of the economic model, that program participation reduces the time available to engage in crime. On the other hand, it is also possible that this is a function of the differing lengths of the in-program periods; the mean number of hours of program participation in Miami was 417, which is roughly equivalent to 14 weeks. We can infer from this that, on the average, the in-program period of 26 weeks for controls was almost twice the length of the average in-program period for experimentals. Similarly the mean number of hours of program participation in New York was 456, which translates to about 15 weeks. These data support the interpretation that the treatment effect may be a function of the variable lengths of the in-program period. Furthermore, in Albuquerque where there was no difference between in-program arrest rates for experimentals and controls, the average length of program participation, 572 hours or approximately 19 weeks, was closest in size to the 26 week in-program period for controls.

The post-program arrest data also offer some support for this interpretation; there was no significant difference between experimentals and controls on likelihood of post-program arrest in any of the sites. See Table 44 for arrest rates. In all three sites, however, experimentals showed a slightly greater likelihood of arrest in the post-program period than did controls.

In addition to the cross-tabulations by site, correlations between treatment and the three arrest variables were computed for the combined sample. While a

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* $X^2=9.25$; df=1; p<.01

** $X^2=7.53$; df=1; p<.01

TABLE 44

Percent Arrested Post-Program

	Site		
	Albuquerque	Miami	New York
Experimental	16%	25%	22%
(Total N)	(295)	(376)	(411)
Control	13%	21%	19%
(Total N)	(323)	(393)	(421)

significant correlation, r=.08, was obtained between treatment and number of in-program arrests, the statistical significance may be attributed to sample size (N=2219); clearly, this relationship is extremely weak. For the relationship between treatment and number of post-program arrests, the correlation was r=-.02, which was not significantly different from zero. Similarly, the correlation between treatment and total number of arrests subsequent to intake was r=-.02 (also not significantly different from zero). Neither the cross-tabulations nor the more powerful correlational analysis provided support for a program effect on number of arrests, either during program participation or post-program. The possibility of program effects on subgroups of AYES participants is discussed below.

Despite the lack of treatment effect on arrest rates, an analysis of variance was computed on the number of post-intake convictions. Since only 515 members of the sample had been arrested during the post-intake period, and even fewer convicted, the period was not divided into in-program and post-program for the analysis of conviction data. The analysis revealed a significant site effect,* but no significant difference between experimentals and controls, and no interaction effect. Among the arrested subjects, New York had the highest mean number of convictions (1.14); this was significantly higher than the mean number of convictions in Albuquerque (0.86) or in Miami (0.81). The site difference parallels that found in the analysis on number of convictions prior to AYES intake, and is probably the result of different adjudictatory processes in the three jurisdictions. The lack of a program effect on convictions is not surprising given the lack of program effect on arrests.

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* $F(2,490)= 6.76; p<.001$

In addition to collecting data on arrest and conviction rates subsequent to intake, detailed data were collected on up to five arrests during this period. Since so few AYES subjects were arrested at all during the period, analysis was confined to the characteristics of their first post-intake arrest. The analyses were computed on the arrested subsample only, and dependent variables included: severity of arrest charge, type of crime, disposition, conviction charge severity, and type of conviction charge. Each of these variables was cross-tabulated with treatment, separately for each site. Of a total of 21 cross-tabulations, there were only two significant relationships with treatment, one of these in New York and the other in Miami.

There was no relationship of treatment with severity or type of arrest charge or likelihood of conviction; however, if convicted, Miami controls were more likely than experimentals to have been convicted on felony charges ($X^2=3.755$; $p<.05$; phi=.23). However, because controls were no more likely than experimentals to have been convicted, the above analysis involved only the 41 convicted controls and the 31 convicted experimentals, not quite 10% of the Miami AYES sample. Therefore, this effect can not be considered a very powerful program effect.

As described in Chapter III, one of the "type of crime" variables categorized charges as either income-producing or non-income producing. In New York, convicted controls (63%) were more likely than convicted experimentals (43%) to have been found guilty of income-producing crimes (X^2=3.82; p=.05; phi=.20). As was the case in Miami, New York controls were no more likely than experimentals to have been convicted, so it would be difficult to claim this as a reliable indicator of program effect.

Considering all the analyses involving treatment, there are no indications that the AYES program reduced the number (or quality) of arrests or convictions. This was true for all three sites. In Section B, the possibility of the three program models having differential impacts is explored.

B. The Three Program Models

Analyses similar to those described in section A were computed to determine model effects, substituting program model for treatment. An analysis of variance on number of post-intake arrests was computed, with the model variable including the control group as a fourth model. As was indicated in Section A, there were significant differences among the sites in the mean number of arrests. There was a significant effect for model (F(3,2174)=5.70;p=.0008); and the results of the multiple comparison test on the means indicated that Model I participants had a significantly higher mean number of arrests than did Model II or III participants or members of the control group. Furthermore, as can be seen from the data in Table 45, this holds true across sites.

As discussed in Chapter III, this model effect was also present in the analysis of number of arrests prior to intake. In fact, it appears that the program was equally ineffective across models; that is, those subjects who had relatively high arrest rates prior to intake continued to have relatively high arrest rates subsequent to entering in AYES. This contention is further supported by the data presented in Section C of this chapter -- once the important predictors of post-intake arrest are entered into a regression analysis, the model effect disappears.

TABLE 45

Mean Number of Post-Intake Arrests

	Site		
	Albuquerque	Miami	New York
Model I	0.45	0.73	0.53
(N)	(89)	(127)	(139)
Model II	0.31	0.25	0.39
(N)	(100)	(112)	(139)
Model III	0.23	0.58	0.25
(N)	(101)	(136)	(133)
Control	0.26	0.63	0.45
(N)	(321)	(383)	(419)

For the cross-tabulations of model with in-program arrests and post-program arrests, the control group members were eliminated. There was no relationship in any of the sites between program model and the likelihood of being arrested while in the program. There were, however, significant model effects in all three sites on post-program arrests. As can be seen from the data presented in Table 46, the nature of the effect differs slightly by site. However, as will be seen in Section C, the model effect does not remain when other covariates are included in the analysis.

TABLE 46

Percent Arrested Post-Program

	Site		
	Albuquerque*	Miami**	New York***
Model I	21%	32%	28%
(Total N)	(91)	(127)	(139)
Model II	19%	12%	22%
(Total N)	(103)	(113)	(139)
Model III	9%	29%	16%
(Total N)	(101)	(136)	(133)

* $X^2=6.18$, df=2, $p<.05$

** $X^2=14.16$, df=2, $p<.001$

*** $X^2=5.99$, df=2, $p<.05$

The number of post-intake convictions was also analyzed for model differences. As was discussed in Section A, there were significant differences among the sites on number of convictions. There were, however, no significant differences among the models, nor was there an interaction between site and model. Thus, although Model I participants tended to be arrested more frequently than the other AYES research subjects, they were no more likely than the other subjects to be convicted on those arrests.

Although cross-tabulations were run on the detailed data on the first arrest subsequent to AYES intake, there were no significant model effects. It appears that the program model in which an AYES subject participated was unrelated to the type or severity of arrest charges, the disposition of the case, or the type or severity of conviction charges.

From the data presented in Sections A and B of this chapter, it is clear that, taken as a whole, neither participation in AYES nor the specific model in which one participated is related to post-intake criminal justice involvement. Therefore, the analyses presented in section C were computed to explore other predictors of post-intake criminal justice involvement and to determine whether there were subgroups who benefitted from the AYES program.

C. Beyond the Experimental Design

1. Predictors of Arrest

Criminological literature suggests a number of predictors of crime. Since subjects were randomly assigned to treatment group (experimental/control), we would expect the groups to be similarly distributed with respect to age, gender, race, arrests prior to intake, and referral source. Such distribution, however, does not eliminate the possibility of differential program effects for males and females or older and younger participants. In addition, some of the model effects could be explained by self-selection factors during the guided choice phase of intake.

A series of multiple regression analyses were computed to determine the effects of prior arrests, demographic variables, and referral source on arrests subsequent to intake. These analyses were also used to determine whether, after controlling for the above-mentioned variables, there was any evidence of treatment effects. In addition, an analysis of variance design was used to test for interaction effects with treatment, which would indicate differential effects of AYES across subgroups.

Inspection of the simple correlations in Table 47 reveals that the best predictor of number of post-intake arrest is number of arrests prior to AYES intake. A regression analysis on number of post-intake arrests with gender, age, referral source, number of prior arrests, site, and treatment as predictors resulted in $R^2=.19$. The results of the regression analysis are presented in Table 48. While there were significant effects for all predictors except treatment, only gender, referral source, and priors accounted for a meaningful percentage of variance. The best predictor was number of prior arrests; even after controlling for gender, age, and referral source, number of prior arrests accounted for approximately 8% of the variance on number of post-intake arrests. Gender accounted for 7% of the variance in number of post-intake arrests. As would be expected, males were arrested significantly more frequently (mean=.62) than were females (mean =.08).* An additional 2% of the variance was accounted for by referral source. It should be noted, however, that referral source entered the analysis before number of prior arrests; had the order of entry been reversed, referral source would not have accounted for even as much as 2% of the variance in post-intake arrests. (Because we were interested in the effect of referral source on arrests, we chose to enter it before priors in this analysis.) In other

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* $F(1,2155)=167.99$; $p<.0001$

TABLE 47

Correlations of Predictors With Dependent Variables*

Predictors		Post-intake Arrests	In-Pgm Arrests	Post-Pgm Arrests	Post-Intake Convictions
Pre-Int. Arrest		.38	.34	.25	.15
	(N)	(2172)	(2174)	(2174)	(496)
Pre-Int. Convict		.31	.26	.23	.17
	(N)	(2149)	(2151)	(2151)	(489)
Gender		-.27	-.18	-.20	-.09
	(N)	(2199)	(2218)	(2218)	(515)
Age		-.10	-.08	-.06	-.04*
	(N)	(2196)	(2215)	(2215)	(515)
Referral Source		.23	.16	.16	.07*
	(N)	(2200)	(2219)	(2219)	(515)
Program Hours		-.23	-.05*	-.22	-.06*
	(N)	(1060)	(1066)	(1066)	(250)
Treatment		.02*	.08	-.02*	.06*
	(N)	(2200)	(2219)	(2219)	(515)

* All correlations are significant at the .05 level or better unless indicated with an asterisk (*).

TABLE 48

Regression Analysis on Number of Post-Intake Arrests

	B	p	Beta
Intercept	1.07	.0001	-
Gender	-.32	.0001	-.16
Age	-.03	.02	-.05
Referral Souce	.13	.003	.07
Number of Priors	.20	.0001	.30
Albuquerque Site Dummy	-.01	NS	-.01
Miami Site Dummy	.18	.0001	.09
Treatment	.04	NS	.02

N=2166

F=71.662; $p < .0001$

R^2=.19

Adjusted R^2=.19

analyses on post-intake arrests, a comparison between regressions including both referral source and number of priors and regressions including number of priors alone revealed no difference in the total R^2. Nonetheless, if number of priors is not controlled, there are significant differences between criminal justice referrals and non-criminal justice referrals in all three sites; the mean number of arrests for criminal justice referrals was .68, as compared to a mean of .24 for non-criminal justice referrals. Age would be expected to be related to number of arrests; it was, although the relationship was very weak (r=-.10). The relationship was in the expected direction; i.e., younger AYES research subjects were arrested more frequently than older subjects. It is likely that the restricted age range (16-21 at intake) served to depress the correlation.

In addition, an analysis of variance was computed on number of in-program arrests; the results of this analysis were very similar to those found for post-intake arrests. There were significant effects for age, gender, referral source, and model. While the age effect was quite weak, the trend was linear, and the results of the multiple comparison tests indicated 16-18 year olds had significantly more in-program arrests than did 19-21 year olds. The analysis of variance results are presented in Table 49, and the means by age, gender, referral source, and model are presented in Table 50. The analysis revealed no significant interactions with age, including the interaction of age by model. There was no evidence of differential program benefit by age. The model effect was quite weak, and disappeared in a regression analysis which included number of priors as a predictor.

TABLE 49

Analysis Of Variance On Number of In-Program Arrests

Source	df	F	p
Age (A)	5	4.13	.001
Site (B)	2	1.93	NS
Gender (C)	1	76.12	.0001
Referral (D)	1	25.33	.0001
Model (E)	3	5.34	.002
AxB	10	<1	NS
AxC	5	1.49	NS
AxD	5	1.14	NS
AxE	15	<1	NS
Error	2174		

Similar analyses were run on number of post-program arrests, with similar results. There were significant effects for age, site, gender, and referral source, as well as an age by referral source interaction. The site effect was similar to that for the total number of arrests since intake; Miami had the highest mean (.44), followed by New York (.31) and Albuquerque (.21). Males had significantly more arrests than females; their means were .45 and .09, respectively. While the age and referral source effects were similar to those discussed above, the interaction between them sheds more light on the actual relationship. The mean number of arrests by age and referral source are presented in Table 51 and the analysis of variance is in Table 52.

TABLE 50

Means For Number Of In-Program Arrests

AGE AT INTAKE

	16	17	18	19	20	21
Mean	.22	.17	.16	.15	.09	.07
N	262	434	493	439	359	223

GENDER

	Male	Female
Mean	.21	.03
N	1473	737

REFERRAL SOURCE

	Criminal Justice	Other
Mean	.23	.08
N	1012	1198

MODEL

	I	II	III	Control
Mean	.14	.08	.10	.18
N	356	355	368	1131

TABLE 51

Mean Number of Post-Program Arrests

Referral Source	16	17	18	19	20	21
Criminal Justice	.56	.53	.53	.58	.22	.33
N	151	233	216	168	149	95
Other	.31	.14	.16	.18	.23	.28
N	111	201	277	271	210	128

The data in Table 51 indicate that there were substantial differences by referral source on number of post-program arrests for the younger members of the sample. However, for the 20 and 21 year olds, referral source did not predict number of arrests. These results are consistent with other evidence that criminal activity tends to decrease with age; by age 20, the criminal justice referrals were being arrested at the same rate as the non-criminal justice referrals. (Note that "age" is age at intake, therefore, some of the "20 and 21 year olds" could be as old as 23 by the end of the follow-up period.) The results in Table 52, however, indicate no effect for model. Regression analyses on experimentals suggest that the introduction of priors as a predictor accounts for most of the

TABLE 52

Analysis Of Variance On Number of Post-Program Arrests

Source	df	F	p
Age (A)	5	2.40	.05
Site (B)	2	12.69	.0001
Gender (C)	1	93.02	.0001
Referral (D)	1	21.14	.0001
Model (E)	3	1.30	NS
AxB	10	1.43	NS
AxC	5	<1	NS
AxD	5	3.36	.005
AxE	15	1.23	NS

explained variance, eliminating age, site, and referral source as significant predictors.

The analysis of post-intake convictions described in Section A of this chapter produced significant site effects, but no effect of AYES participation. To test for effects of covariates on this variable, a regression analysis was run on the sample of arrested AYES subjects (N=495). With age, gender, referral source, number of arrests prior to intake, site (dummy variables), and treatment as predictors, only 6% of the variance in number of post-program convictions was explained. The only significant effects were for number of prior arrests and site, each accounting for about 2% of the variance. Even after controlling for gender, age, referral source, and priors, New York AYES subjects had more convictions than those in the other two sites. However, there remained no evidence of a treatment effect. The results of the regression analysis are presented in Table 53.

The results of the analyses on post-intake arrests, in-program arrests, post-program arrests, and post-intake convictions are all consistent. Throughout these analyses, prior arrests and gender are the primary predictors. There are no treatment effects, even after controlling for the covariates, nor are there any interactions with treatment. Thus, these results provide no support for the hypothesized effect of AYES on criminal justice system involvement subsequent to the program.

A series of similar analyses were conducted on the experimental subsample. These analyses were done to determine whether the model effects described in Section B could be attributed to AYES, or were the result of selection factors. As part of this analysis, a variable identifying the process used for assignment to program model (guided choice or random assignment) was entered in the analysis.

TABLE 53

Regression Analysis On Number Of Post-Intake Convictions

	B	p	Beta
Intercept	.33	NS	-
Gender	-.18	NS	-.05
Age	.03	NS	.05
Referral Souce	.07	NS	.04
Number of Priors	.07	.001	.15
Albuquerque Site Dummy	-.20	NS	-.09
Miami Site Dummy	.30	.002	-.16
Treatment	.12	NS	.06

N=495

F=4.16; P<.0001

R^2=.06

Adjusted R^2=.04

The data presented in Chapter III indicated that participants in Model I had significantly more arrests prior to intake than did participants in the other two models. Data presented in Section B of this chapter suggested that Model I participants also had significantly more arrests subsequent to AYES than did participants in Models II and III. Since over half the participants were assigned to program models through a method involving their own choice and the counselors' evaluation of their needs (guided choice), it is possible that the model effects were due to the selection process rather than to experiences in the program. To test this possibility, analyses of variance were run on number of arrests subsequent to intake, number of in-program arrests, and number of post-program arrests. In each of these analyses, the independent variables were site, assignment type, model, and the interactions among the independent variables. For number of arrests post-intake and number of arrests post-program, there were no significant main effects or interactions involving assignment type. The site and model effects for the entire sample were described above, and are the same for the subsample of experimentals. There was a significant model by assignment type interaction ($F(2,1062)=3.06; p<.05$) on number of in-program arrests; however, since all variables in the analysis accounted for a total of 2% of the variance, this effect is not very meaningful. Therefore, the combined results of these three analyses provide no evidence that there were differential effects of the program for those who came into it through guided choice versus random assignment.

A series of regression analyses were run to determine whether the model effects could be attributed to AYES. The results of these analyses indicated that the best predictors of arrest were prior arrest history and gender. Age had no appreciable effect, nor after number of prior arrests was controlled, did site. Most important, the model effects disappeared when prior arrests were controlled, implying that the model effects discussed in Section B were due to pre-existing characteristics of the participants.

The regression on number of arrests during the entire post-intake period produced the highest multiple correlation, $R^2=.19$. Of the explained variance, approximately 8% was due to gender and 10% to number of arrests prior to AYES participation. In this analysis, the variance which had been attributed to referral source was explained by number of priors and gender, as was that previously attributed to site and model. The results of this analysis are presented in Table 54.

TABLE 54

Regression Analysis On Number Of Post-Intake Arrests Of Experimentals

	B	p	Beta
Intercept	.59	.0001	-
Gender	.33	.0001	-.17
Prior Arrests	.18	.0001	.29
Referral Source	.11	.05	.06
Albuquerque Site Dummy	.01	NS	.01
Miami Site Dummy	.14	.02	.07
Model I Dummy	.06	NS	.03
Model II Dummy	-.03	NS	-.02

N=1061

F=34.69;$p<.0001$

R^2=.19

Adjusted R^2=.18

Analyses on number of arrests while in the program and on number of post-program arrests produced similar results. Prior arrests and gender were the best predictors, each explaining approximately 3% of the variance in number of in-program arrests and 6% of the variance in number of post-program arrests. The major differences were in the total percent of variance explained by the predictor variables; for in-program arrest, the R^2 was only .07, and for post-program arrests it was .13. Much of this reduction is probably due to restriction of range; there were very few arrests during the in-program period, and therefore, most of the 1061 participants had values of zero on this variable. These results indicate that the great bulk of the variation on arrests subsequent to AYES participation remains unexplained, and the explainable variance is not related to the specific program model in which the participant was placed.

2. Employment and Crime

In a review of the literature on relationships between employment and crime, Thompson et al. (1981) cited research evidence of a relationship between time spent working and arrest rates. Friedman (1978) reported a relationship between stability of employment and arrest rates for a sample of ex-addicts who were the subjects of an evaluation of a supported work progam. This study followed members of the experimental and control groups for three years, and found arrest rates to be significantly lower for the "more steadily employed" than for those members of both groups who worked less during the three-year period. Subjects who were employed more than 18 out of 36 months were arrested an average of .22 times per year, while those who worked less than 18 out of 36 months were arrested an average of .48 times per year. Similar results were found from the LIFE (Lenihan, 1977) and TARP (Rossi et al., 1980) studies on prison releasees; both studies found strong relationships between being employed and reduced arrest rates.

The results of these studies led to the hypothesis that among AYES research subjects, employment would be related to reduction in arrest rates. To test this hypothesis, multiple regression analyses were computed on each of the post-intake arrest variables. The predictor variables were age, gender, highest grade completed in school, marital status, number of arrests prior to AYES intake, site, and treatment. These are the same predictor variables used in the analyses discussed above. In addition, each analysis contained either weekly earnings on the most recent job and percent of the follow-up period spent working or total earnings during the follow-up period. The employment variables were entered last to determine the effect of employment on arrests with all other variables controlled.

The analyses on number of post-intake arrests produced a squared multiple correlation of $R^2=.19$; the results were the same for the equation containing weekly earnings and percent of time worked as for the equation containing total earnings. Furthermore, as was presented in Table 48, non-employment variables accounted for 19% of the variance; since the addition of employment variables accounted for no additional variance, the hypothesis that time working would be negatively related to arrests was not supported by the data. When similar analyses were run for experimentals only, adding number of hours of AYES participation, the squared multiple correlation was .21. Program hours accounted for 2% of the variance in post-intake arrests; the more time an experimental spent in AYES, the less likely he/she was to be arrested at some time during the 14 months subsequent to intake. The relationship of program hours to both employment and crime outcomes is discussed in greater detail in Chapter VII.

The analyses on number of in-program arrests and number of post-program arrests were computed for experimentals only. The predictor variables were the same as for total arrests subsequent to intake; however, program model (I, II, or III) was substituted for treatment, and program hours was included in the analysis. The regression analysis on number of arrests while in the program yielded a squared multiple correlation of $R^2=.06$. The only significant predictors were gender, accounting for 3% of the variance, and number of prior arrests, which accounted for 2% of the variance. Neither program hours nor any of the employment varibles significantly predicted how many times a participant was arrested while in AYES.

The regression analyses on number of arrests after leaving the program each produced squared multiple correlations of .14. The best predictors of post-program arrests were gender (6% of the variance), number of arrests in the two years prior to AYES participation (4% of the variance), and hours of AYES participation (2%). None of the other variables contributed significantly to the prediction of post-program arrests.

The results of the analyses on post-intake, in-program, and post-program arrests provide no indication of a relationship between employment and crime for AYES research subjects. The best predictors of post-intake crime were gender and prior arrests. The reasons for the lack of relationship between employment and crime are unclear. It is possible, however, that the low rate of employment for AYES subjects during the follow-up period, combined with their relatively low arrest rate, resulted in effects too small to be detected. In other words, had the range of number of arrests been larger, or had there been more employment, there might have been some relationship between these variables. While the AYES results are different from those reported by Friedman (1978), Lenihan (1977), and Rossi et al. (1980), the population from which the AYES subjects were drawn was different than those of the other studies. For example, the average age at intake of AYES subjects was 18.8; in contrast, the average Wildcat worker was 31 years old at program intake (Friedman, 1978) and the average TARP participant was in his late twenties. Furthermore, the criminal histories of the Wildcat and TARP participants tended to be more extensive. While differences in the populations do not explain the differences between the results of these studies and the AYES study, such differences should be considered.

In summary, the analyses presented in this chapter provide no evidence of a treatment effect on subsequent criminal justice system involvement. Furthermore, the apparent model effect presented in Section B disappeared entirely when prior arrests was entered as a covariate. The only program variable which continued to predict arrests subsequent to AYES intake was number of hours spent in the program. Because one measure of program success is post-program arrests, and number of hours spent in the program predicts arrests, it would be useful to understand what predicts hours of AYES participation. This and other measures of program success are discussed in the chapter that follows.

CHAPTER VII: PREDICTORS OF PROGRAM SUCCESS

The analyses on outcome measures presented in Chapters IV, V, and VI suggested little effect of participation in the AYES program. There were no differences between experimentals and controls on the SAS post-tests, even after controlling for pre-test scores, gender, and ethnicity. AYES participants were somewhat more successful (51%) than control group members (41%) in obtaining post-program employment; they did not, however, get higher paying jobs or keep them longer. AYES did not appear to have any impact on recidivism: experimentals and controls were equally likely to be arrested post-intake.

The only program variable that seemed to have an effect on post-intake arrests was number of hours of AYES participation. The more hours a participant spent in AYES, the fewer times he/she was likely to be arrested post-intake or subsequent to leaving the program. Because this effect was significant even after controlling for number of prior arrests and gender, it raises the question of how to identify groups of participants who tended to stay in the program relatively longer than others. Staying in the program is some indication that the participant is able to follow the rules, come to work on time, accept supervision, and refrain from fighting with the other participants. A related, short-term measure of program success is positive termination from the program; an AYES participant could receive a positive termination by completing the full 26 weeks, being placed in a job, or being placed in another training program or school. Reasons for negative termination included refusal to continue the program (dropping out), disciplinary action by the program, or termination as the result of arrest and incarceration.

The analyses presented in this chapter focus on the determinants of number of hours of program participation and program sucess as measured by termination status, the effects of program participation on employment and crime outcomes, and the effects of family background, social situation, and education on these outcomes. The family background data were collected on the Vera Intake interview and, while they fall outside the experimental design, may shed some light on who succeeds in a program like AYES. Clearly, these analyses are exploratory, and any interpretations of them should be made with caution.

A. Predictors Of Program Participation

Although each participant was entitled to 26 weeks of AYES participation, the experimental group was composed of individuals who spent anywhere from one hour to the full 26 weeks in the program. It is possible that one reason for the lack of differences between experimentals and controls on post-program outcomes is that many of the experimentals received little treatment; that is, they left the program after a very short time. For this reason, time spent in AYES was treated as a dependent variable in an analysis of the predictors of length of participation.

As was discussed in earlier chapters, Model I participants spent 35 hours per week in AYES participation, while Model II and III participants were paid for 30 hours per week. The reason for this difference was to equalize the take-home pay of Model I participants, who received taxable wages, and Model II and III participants, who received non-taxable stipends for the time spent in classroom training. Thus, a Model I participant who spent the full 26 weeks in the program, with no absences would participate for 910 hours, while a Model II or III participant with no absences would spend 780 hours in AYES. In fact, few participants were in the program for that long; the average number of hours in the program was 497 for Model I participants, 472 for Model II participants, and 455 for Model III participants.

An analysis of variance was computed on total number of hours of AYES participation to determine which variables were the best predictors. The independent variables in the analysis were ethnicity (Black vs. Hispanic), site, gender, referral source, and age. As can be seen from Table 55, there were significant effects for each of these variables. Albuquerque participants stayed in the program the longest, with a mean of 575.7 hours; New York participants spent an average of 455.4 hours in the program, and Miami participants were in the program for a mean of 419.5 hours. In addition, females were in the program longer (with a mean of 529.2 hours) than were males (mean=440.0 hours). This effect was independent of site; that is, in each of the three sites, females stayed in the program longer than did males. Thus, the site effect cannot be attributed to the greater proportion of females in Albuquerque.

Hispanics, with a mean of 517.9 hours in the program, participated significantly more than Blacks, whose mean was 437.4 hours. Unfortunately, because of the ethnic composition of the three sites, the relationship of race and site is not easily interpreted. In both New York and Miami, Hispanics and Blacks spent approximately equal amounts of time in the program. In Albuquerque, where program participation was greatest, there were only 19 Blacks. It appears that the ethnicity effect is probably more accurately interpreted as a manifestation of the site difference: 53% of the 432 Hispanic participants were from Albuquerque, and the mean number of hours Albuquerque Hispanics spent in AYES was 582, as compared to 423 for Miami Hispanics and 462 for New York Hispanics. It is impossible to tell from these data whether, had there been more Blacks in the Albuquerque AYES program, there would have been a difference between Blacks and Hispanics on program participation.

Criminal Justice referrals spent significantly less time in AYES (mean=417.8) than did non-criminal justice referrals (mean=520.9). In addition the older, 19-21 year old, AYES participants spent significantly more time in the program than did the 16-18 year olds. Thus, the results of this analysis indicate that participants from Albuquerque spent the most time in the program, and that females, Hispanics, non-criminal justice referrals, and older participants were in the program longer than males, Blacks, criminal justice referrals, and younger participants.

TABLE 55

Analysis Of Variance On Program Hours

Source	df	F	p
Ethnicity	1	26.49	.0001
Site	2	18.50	.0001
Gender	1	23.35	.0001
Referral	1	19.74	.0001
Age	5	4.24	.0009
Ethnicity x Site	2	<1	NS
Gender x Site	2	<1	NS
Error	982		

One measure of program success is termination status, positive or negative. Given the results of the analysis on program hours, we would expect Albuquerque to have the highest percentage of positive terminations, and that more Hispanics than Blacks, more females than males, and more non-criminal justice referrals than criminal justice referrals would be positively terminated. To test these hypotheses, separate cross-tabulations with termination status were computedon each of these variables. The results indicated that a higher percentage of females than males were positively terminated, and a higher percentage of non-criminal justice referrals than criminal justice referrals were positively terminated. Ethnicity had no relationship, independent of site, with termination status. While Albuquerque had the highest positive termination rate (66%), New York (49%) and not Miami (56%), had the lowest positive termination rate. The higher positive termination rate for Miami participants is likely a reflection of the institution of a requirement by the prime sponsor that the Miami program have a goal of 80% positive terminations. (See Appendix A for further discussion.) Since the possibility of continuation of AYES by the prime sponsor hinged on the rate of positive terminations, the Miami program was discouraged from making negative terminations.

Thus, with these very narrow definitions of program participation and program success (termination status), we would conclude that females, older participants, Albuquerque participants, non-criminal justice referrals, and perhaps Hispanics were more successful in AYES than were their counterparts. The next logical question is whether this level of program success translated into greater employment benefits or reduction in crime.

B. Effects Of Program Participation On Employment And Crime Outcomes

Knowing which groups of AYES subjects spent the most time in the program, analyses were conducted to determine whether hours of program participation were related to employment and crime outcomes. The employment variables were the percent of the follow-up period spent in employment and the weekly earnings on the most recent job; these data were analyzed for the experimental group as a whole and for those experimentals who worked during the period. These two sets of analyses provide information on which members of the experimental group obtained jobs, and whether any subgroups got better jobs (as measured by wages). The crime variables were number of arrests subsequent to intake, number of arrests while in the program, and number of arrests subsequent to leaving the program.

1. Employment

If those groups of subjects who tended to stay in the program for relatively longer than others were the same subjects who had better employment outcomes, we would expect Albuquerque participants, females, and those referred by agencies not part of the criminal justice system to have more positive employment outcomes. Based on the analysis of variance on percent of time employed, Albuquerque subjects worked more than those from Miami and New York, and non-criminal justice referrals worked more than participants referred by criminal justice system agencies. Of all the experimentals, those from ALbuquerque worked an average of 32.8% of the follow-up period, as compared to means of 26.3% in Miami and 20.6 in New York. Participants referred from criminal justice agencies worked an average of 22.4% of the follow-up period, while all other participants averaged 29.3% of the period. Thus the relationships of referral source and site to percent of time employed are consistent with their relationships to hours of program participation. On the basis of program participation, we would also expect females to be employed a greater percentage of the time than male AYES participants. This hypothesis was not supported; rather, the effect was significant and in the opposite direction. The mean percent of time working for males was 29.1 as compared to a mean for females of 21.0. Thus, despite greater program participation, females spent less time employed during the follow-up period. The results of the analysis of variance on percent of time working are presented in Table 56. In addition to working more, males had significantly higher weekly wages (mean = \$71.21) than did females (mean = \$47.38).* Taken together, the results on percent of time working and weekly wages suggest that males were more likely to have worked than were females. This conclusion is also supported by the results of the logistic regression presented in Chapter V, Table 36. There were no other significant effects on either percent of time working or weekly earnings.

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* $F(1,675)=15.60$; $p<.001$

TABLE 56

Analysis Of Variance on Percent of Time Working (Experimentals Only)

Source	df	F	p
Site	2	6.21	.002
Model	2	<1	NS
Gender	1	9.40	.002
Ethnicity	1	<1	NS
Referral	1	10.80	.001
Error	675		

The percent of time working and earnings data were also analyzed for the subsample of experimentals who worked at some time during the follow-up period. These analyses were used to determine whether there were subgroups of AYES participants who obtained better jobs in terms of weekly wages, and whether they tended to work more than other subgroups of participants. The only significant effect on percent of time working was that of referral source*; criminal justice referrals (who worked during the follow-up period) worked an average of 50.7% of the period, while other AYES participants worked an average of 58.3% of the period. This implies that non-criminal justice referrals not only stayed in AYES longer than criminal justice referrals, but also were employed for a greater percentage of time during the follow-up period. There was no evidence that the non-criminal justice referrals found higher paying jobs, however; the effect of referral source on earnings was non-significant for both the experimental group as a whole and the working subsample.

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* $F(1,312)=4.64$; $p=.03$

The results of the analysis of variance on weekly earnings of those AYES participants who worked subsequent to leaving the program indicated significant effects for site, model, and gender, and an interaction between site and model. While the analysis on the total sample of experimentals indicated that Albuquerque participants had the highest mean weekly earnings, the analysis on the working subsample indicated that they had the lowest mean weekly earnings.* The average weekly salary for those Albuquerque participants who worked was $123.09, as compared to a mean of $143.09 for working Miami participants, and $151.07 for New York participants. The analysis on the total sample reflects the percentages of experimentals in each site who worked during the follow-up period (64% in Albuquerque, 47% in Miami, and 44% in New York), and the analysis of the working subsample reflects the wages they earned on those jobs. There was also a significant model effect on wages of working participants**; Model I particiants, with an average weekly wage of $148.82 had significantly higher wages than Model II (mean = $133.12) or Model III (mean = $134.92) participants. However, the significant interaction effect indicated that this difference was present only in Miami and New York.*** The means for weekly wages of working participants are presented in Table 57 below. Thus, wages in Albuquerque were relatively low for participants from all three models, while Model I participants in Miami and New York tended to have higher weekly wages than Model II and III participants in those sites. It is possible that, while we do not have data to test the hypothesis, Model I participants in Miami and New York were more likely than those in Albuquerque to get jobs in construction, which might be higher paying than other jobs available to these participants.

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* $F(2,296)=7.81$; $p<.0005$

** $F(2,296)=2.94$; $P<.05$

*** $F(4,296)=2.56$; $p<.05$

TABLE 57

Mean Weekly Earnings for Working Experimentals

	Site		
	Albuquerque	Miami	New York
Model I	$118	$156	$178
(N)	(34)	(34)	(27)
Model II	126	133	143
(N)	(36)	(37)	(26)
Model III	124	142	135
(N)	(35)	(53)	(32)

As was discussed above, in the experimental sample as a whole, male AYES participants had higher weekly earnings than did female AYES participants; this effect was significant for the working subsample also.* The average weekly wages on the most recent job were $143.69 for males and $126.17 for females. Since gender entered the analysis after site, site was controlled; therefore, the difference between males and females is not attributable to there being an over-representation of females from Albuquerque. It appears that the jobs obtained by AYES females were lower paying than those obtained by AYES males. This was true across models; males from Model I earned more than females who had been in Model I, and males who had participated in Models II and III earned more than females who were in those models. We can conclude from these results that staying in AYES longer than the males did not help the females get as many jobs as males; nor did it help them get jobs that paid as well.

Having considered the effects of site, model, gender, and referral source on number of hours spent in the program, and determined which of those had corresponding effects on employment outcomes, regression analyses were computed to determine whether age at program entry and number of hours of program participation had significant effects on employment outcomes. Even with all of these predictors, it was not possible to explain more than 8% of the variance on either of the two employment variables. Furthermore, neither age nor program hours contributed anything to explaining variation in wages of the entire experimental group or the working subsample.

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* $F(1,296)=4.14$; $p<.05$

In the analysis on percent of time working for the entire sample of experimentals, 7% of the variance was explained. As was discussed above, there were significant effects for site, gender, and referral source. In addition there were significant effects for age and program hours, each explaining an additional 1% of the variance in percent of time worked. Older participants tended to work a greater percentage of the time than younger participants, and the longer the time spent in AYES, the greater percentage of the follow-up period participants tended to work.

Because these results might reflect differences in the likelihood of obtaining work, the same regression analysis was computed for the subsample of experimentals who worked at some time during the follow-up period. This analysis also produced a squared multiple correlation of .07, with significant effects for age and program hours. Age explained about 2.6% of the variance and program hours explained about 2% of the variance in percent of time worked. As would be expected, older participants and those who spent more time in AYES worked more than younger participants and those who spent relatively less time in the program. While the effect of time spent in the program is not large, it is significant even after controlling for site, model, gender, age, and referral source. Thus, there is some evidence that staying in AYES had positive effects on percent of the follow-up period spent working. Perhaps if some of the participants who dropped out early could have been induced to remain in the program, they would have experienced more positive employment outcomes. On the other hand, because number of hours spent in the program is clearly not random, there may be outside factors, not tested for in this research, that influenced both length of stay in AYES and percent of time employed after leaving the program. For example, a participant with a drug problem might have problems both with the program and with keeping a job. Child care problems, housing problems, or medical problems might have similar effects.

2. Crime

As discussed in Chapter VI, the best predictors of arrest subsequent to AYES intake were gender and arrests prior to AYES. The regression analyses for the experimentals were expanded to include hours of program participation as a final predictor of arrests. Program hours could reduce arrests either as a direct result of AYES experiences or indirectly; that is, there may be some participant characteristics which served to increase the likelihood of staying in the program and also decreased the likelihood that they would be arrested. Regression analyses were computed on number of arrests subsequent to AYES intake, number of arrests while in the program, and number of arrests subsequent to leaving the program.

Because the analyses are discussed in Chapter VI, the discussion in this chapter will be limited to the effects of number of hours of program participation on the dependent variables. Program hours was significantly correlated with number of post-intake arrests ($r=-.23$) and with number of post-program arrests ($r=-.22$), but was not significantly correlated with number of arrests while in the program ($r=-.05$). Thus, the longer a participant stayed in the AYES program, the less likely he/she was to be arrested at any time after starting the program, and the less likely he/she was to be arrested after leaving the program.

When hours spent in the program was added to the regression analysis on number of arrests subsequent to AYES intake, the R^2 increased from .19 to .21; thus 2% of the variance in post-intake arrests was explained by hours spent in the AYES program. This effect is significant after controlling for gender and number of arrests prior to AYES intake, and is an indication of an effect, albeit weak, of program participation. The results of the analysis of post-program arrests were similar; the R^2 was .14, with 2% of the variance due to hours spent in the program. Again, this effect was significant even after controlling for gender and number of priors.

Number of hours spent in the program contributed nothing towards explaining in-program arrests. Even with the full complement of predictor variables (described in Chapter VI), the squared multiple correlation was .06. We are able to explain very little of the variance in in-program arrests, and none of it is due to hours spent in the program. Part of the problem with this variable may be that in-program arrests were so infrequent: the mean number of in-program arrests was .10, and therefore, the range is quite restricted.

These results imply that increasing the length of participation in the program might decrease the number of post-program arrests, but would not affect the number of in-program arrests. It is impossible to tell whether this is a socializing effect of the program or something more indirect. In the prior section, we indicated that length of stay in AYES was a predictor of percent of the follow-up period employed, and here we report that it is also a predictor of post-program arrests. Since there was no correlation between percent of time working and number of post-program arrests, we cannot attribute the reduced arrest rate to increased time spent working. It is more likely that there are some unmeasured factors that determine how long a participant stays in a program like AYES and also determine the likelihood of arrest after leaving the program. We cannot tell from the available data whether these factors could be changed, to increase the length of stay in AYES, for example. Thus, while knowing how long a participant stayed in the program provides us with some additional information about the likelihood of arrest during the follow-up period, the reasons for the relationship remain unexplained.

C. Effects of Family Background and Current Social Situation on Employment and Crime Outcomes

The results of analyses presented in Chapter V indicated that the best predictors of employment outcomes were treatment (experimental/control), site and gender. In Chapter VI the results of the analyses on post-intake criminal justice involvement were presented. These analyses indicated that gender, referral source, and number of arrests prior to AYES intake were the best predictors. In Section B of this chapter, analyses of the effects of program participation on employment and crime were presented. The results of these analyses indicated that where there were effects, they were small.

In an attempt to determine whether there were characteristics of subjects' adolescence which would predict employment and crime outcomes, a series of exploratory analyses were computed. The predictors in these analyses were variables indicating: whether the subject came from an intact (two-parent) family, whether there was a working father present, whether any family members had ever been arrested, status of parents' occupation(s), and welfare history of the family. There were no significant effects for welfare history, family arrest history, having a working father, or parents' occupational status. There were some significant, though very small, effects for family composition (intact or not). These results, therefore, should be considered purely exploratory.

AYES subjects whose families were intact -- two biological parents -- were compared to all other AYES subjects on number of arrests subsequent to intake, post-program earnings, and percent of the follow-up period employed. There was no difference between the mean number of arrests for subjects from intact families and those from non-intact families. There were some employment effects, however. In the sample as a whole, subjects from intact families worked significantly more (F(1,1316)=10.45; p=.001) than did those from non-intact families. There was also a significant interaction of family composition with gender (F(1,1316)=5.94; p=.01); from examination of the means in Table 58, we can see that the effect of family composition was for males only. That is, males from intact families worked more than males from non-intact families, while family composition had no effect on percentage of time working for females. It is possible that an intact family offers a youth a wider network to use in securing employment. However, because the expanded network primarily reflects the father's contacts, they may be particularly effective only for male youths.

Table 58

Percentage of Follow-up Period Employed

Family Composition	Gender	
	Male	Female
Intact	32.08	19.02
(N)	(317)	(180)
Non-Intact	21.69	18.42
(N)	(541)	(297)

It appears from the analyses described above that, other than coming from an intact family, the subjects' family background had little effect on their post-AYES employment and crime experiences. It would be likely, however, that their social situation at the time of the eight-month follow-up interview would be related to employment and crime outcomes. Specifically, marital status and having children were used as predictors of post-intake arrests and post-program employment. Marital status differentiated those subjects who were married and living with their spouse from all others, and was, therefore, a dichotomous variable. Similarly, although we had collected information on financial support, there were too few cases to use detailed information about the subjects' relationships with their children. Therefore, the variable on children was also dichotomous, indicating only whether or not the subject had any children.

Marital status was unrelated to post-intake arrests. Among those who received an 8-month follow-up interview, the overall interaction between gender and having children was not significant; however, after adjusting for age, males with children had significantly more arrests than those without children ($p=.02$). The adjusted means are presented in Table 59.

Table 59

Mean Number of Post-Intake Arrests
(Adjusted for Age)

Have Children	Gender Male	 Female
Yes	.72	.09
(N)	(178)	(180)
No	.54	.06
(N)	(691)	(306)

While at first glance these results seem counter-intuitive, it is important to remember that these young men indicated only that they had children; they were not necessarily living with those children. Thus, among this sample, having children may not be an indicator of stability, especially if we control for age. Rather, those males who had children may have been less responsible than those who did not.

Being married and having children (independently) had effects on all the employment outcomes. For the sample as a whole, married subjects had significantly higher weekly earnings (mean=$78.25) than did subjects who were not married and living with their spouses (mean=$53.88).* In addition, married subjects worked a higher percentage of the follow-up period (mean=34.8) than did other subjects (mean=21.9).** The overall gender by marital status interaction effect was not significant for either of these variables; however, there were significant and interesting mean differences after adjusting for age. With age as a covariate, the adjusted mean weekly earnings for married males was significantly higher than other male AYES subjects (p=.003). While there was a similar trend for females, the difference was nonsignificant. The adjusted means on weekly earnings are presented in Table 60(A). Similarly, the overall interaction effect on percent of time working was non-significant; but there were significant differences between married and other males (p=.0006) and between married and other females (p=.05). (See Table 60(B).)

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*F(1,1362)=15.30;p=.0001

**F(1,1362)=16.97;p=.0001

Table 60

(A) Adjusted Mean Weekly Earnings

Marital Status	Male	Female
	Gender	
Married	$103.84	$54.44
(N)	(48)	(60)
Other	70.31	36.48
(N)	(829)	(432)

(B) Adjusted Mean Percent of Time Employed

Marital Status	Male	Female
	Gender	
Married	44.5%	25.2%
(N)	(48)	(60)
Other	27.1	16.0
(N)	(829)	(432)

The effects on employment variables of having children were similar for males, but not for females. There was a significant effect on weekly earnings of the interaction between gender and having children ($F(1,1362)=16.03$; $p=.0001$); males with children earned more than males without children, while females with children tended to earn less than females who didn't have children. (The effect was the same for the unadjusted means as it was after adjusting for age.) This differential effect for males and females probably reflects the parent with whom the children tended to live. It is likely that most children lived with their mothers, making it more difficult for the females with children to work outside the home. Because this analysis was on the entire sample interviewed eight months after exit, the lower wages for females with children may reflect a lower proportion of this group who worked.* The means for weekly earnings are presented in Table 61.

Table 61

Mean Weekly Earnings, for Subjects with Children, and Others

Have Children	Gender Male	 Female
Yes	$89.10	$35.30
(N)	(181)	(182)
No	58.00	43.50
(N)	(696)	(310)

There was a similar interaction effect on percentage of follow-up period employed.** Males with children worked more (mean =34.8%) than other males (mean =22.9%), while females with children worked less (mean =13.8%) than other females (mean =21.3%). Again, it is likely that this interaction reflects a higher proportion of children living with their mothers than with their fathers, thereby creating childcare problems for the females in this sample who had children.

Employment data were also analyzed for the subsample of AYES subjects who had at least one job during the follow-up period. Among both male and female working subjects, those who were married and living with their spouse earned significantly more per week (mean =$150.92) than subjects who were not married (mean=$134.56).* (However, since there were only 56 married subjects in working subsample, these results should be interpreted cautiously.) In addition, married subjects in this subsample worked a significantly higher percentage of the period (mean =71.0) than did non-married subjects (mean =51.1).** In addition, the results of the analysis of covariance indicated that, after controlling for age, the effect of marriage was larger for males than it was for females. That is, married males worked an average of 74.1% of the follow-up period, as compared to an (adjusted) mean of 51.6% for unmarried males; this difference is significant at the .0006 level. Married females worked an (adjusted) average of 63.2% of the follow-up period, while the adjusted mean for unmarried females was 49.0%; this difference is significant at the .05 level.

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* It should be noted that in all of these analyses on post-intake arrest and employment, there were gender differences. Males had significantly more post-intake arrests, significantly higher weekly earnings, and worked significantly more than did females. Because the gender effects are presented and discussed in Chapters V and VI, they will not be discussed further here, except as they interact with other variables.

** $F(1,1362)=18.55$; $p=.0001$

For the working subsample, most of the effects of having children disappeared. The only significant effect was that AYES subjects with children earned significantly more per week (mean = $144.65) than those who didn't have children (mean =132.94).*** The absence of an interaction between having children and gender supports the hypothesis that female AYES subjects had their children living with them, which reduced their ability to work. Among those who worked, females with children earned slightly (though not significantly) more than females without children. There was no effect of having children on percent of time working.

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* F(1,554)=6.47; p= .01

** F(1,585)=15.74; p=.0001

*** F(1,554)=3.68; p=.05

D. Effects of Education on Employment and Crime Outcomes

In the analyses (presented in Chapter V) of human capital hypotheses, highest grade completed in school was used to represent education level. Grade level was chosen over having a high school diploma in an effort to isolate human capital from credentials. We also recognized, however, that for a truncated sample with a median grade level of 10, having a high school diploma might be a more meaningful measure of education. That is, the difference between 10.0 and 10.5 years of education is likely to be less important for employment opportunities than the difference between having a high school diploma and not having one (regardless of grade level completed).

Therefore, a series of analyses were computed on employment outcomes and arrests subsequent to intake, using having a diploma (at intake) as a measure of education. The employment variables were weekly earnings on the most recent job since exit and percent of the follow-up period spent in employment; these analyses were done on the sample as a whole and on the working subsample.

Having a high school diploma had a significant effect on weekly earnings* and on percent of time employed** for the sample as a whole. The 329 high school graduates earned more ($69.31) per week than the 1025 subjects AYES who had not received a diploma prior to intake ($51.30). Similarly, the high school graduates worked a significantly greater percentage of the follow-up period (31.4%) than those without diplomas (20.1%). There was also a significant interaction effect with treatment on percent of time working.*** (See Table 62.) These results suggest that experimentals who entered AYES with a diploma worked a greater percentage of the follow-up period than either experimentals without diplomas or members of the control group.

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* F(1,1342)=15.18; p=.0001

** F(1,1342)=28.89; p=.0001

*** F(1,1342=4.13; p=.04

Table 62

Interaction between Diploma and Treatment on Percent of Time Working

	Experimental	Control
Diploma	37.7	23.6
(N)	(181)	(148)
No Diploma	22.5	17.4
(N)	(546)	(479)

While this effect is weak, it suggests that those participants who came into the AYES program with high school diplomas benefitted from the program, while those participants without diplomas did not work any more than control group members.

The analyses on the working subsample produced a significant effect of diploma on percent of time working.* High school graduates worked significantly more (mean=58.9; n=175) than did those without diplomas (mean=50.2%; n=410). While there was no main effect on weekly earnings, there was an interaction with treatment.** As can be seen from the data in Table 63, the effect is somewhat puzzling: controls who had a diploma at intake earned less during the follow-up period than either controls without a diploma or experimentals. We can only speculate on the reasons for this effect, and must bear in mind that the effect is quite weak (and that there are only 65 working control group members with diplomas).

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* F(1,573)=8.29; p=.004

** F(1,540)=3.80; p=.05

Table 63

Interaction between Diploma and Treatment on Weekly Earnings

(Working Subsample)

	Experimental	Control
Diploma	$139.31	$121.47
(N)	(107)	(65)
No Diploma	$137.04	$140.33
(N)	(226)	(154)

While the three-way interaction effect between Diploma, Treatment, and Site was not tested, we do know that experimentals and controls from Albuquerque were more likely than those from the other sites to be employed; that research subjects from Albuquerque were more likely than those from the other two sites to have a diploma; and that among employed subjects, earnings were lowest in Albuquerque (with a mean of $120, as compared to $144 in New York and $147 in Miami). Thus, it is quite likely that a disproportionate number of those 65 controls with diplomas were from Albuquerque. While the experimentals with diplomas are also likely to be from Albuquerque, with a larger number of cases (154), the mean would be less affected. This interpretation is supported by the lack of a corresponding interaction effect on percent of time working. Among the working subsample, controls with diplomas worked as much (mean=53.8% of the period) as controls without diplomas (mean=49.0) and experimentals without diplomas (51.1). While experimentals with diplomas had a slightly higher mean (62.0), the effect did not approach significance (F<1).

Together these analyses suggest that having a diploma is of some benefit to members of this population. At the very least, the analyses on the sample as a whole suggest that having a diploma makes it easier to get a job. Furthermore, there is some evidence (from the interaction with treatment) that the AYES program enhanced that effect. The percent of the follow-up period wored by controls with diplomas was 23.6 and by those without diplomas was 17.4; these numbers are virtually identical to the percent of the year prior to intake worked by research subjects with diplomas, 23.1 (N=503) and those without diplomas, 16.9 (N=1676). Being in the program, even without a diploma, seemed to have some benefit -- after leaving the program, the experimentals without diplomas worked about as much (22.5) as controls with diplomas.

The analysis on number of post-intake arrests suggests that having a diploma was related to the number of times males were arrested, but not related to the number of arrests for females. There was a significant gender by diploma interaction* on number of post-intake arrests. The mean number of post-intake arrests for male high school graduates was .41 (N=261), as compared to a mean of .68 for males without a diploma (N=1169). Female high school graduates had a mean of .05 arrests (N=235) and those without diplomas (N=495) had a mean of .10 arrests. It is clear that this is a weak effect; however, there was a similar (and stronger) effect on number of arrests prior to intake.** The mean number of arrests prior to intake for male high school graduates was .58 (N=257), as compared to 1.17 for males without diplomas (N=1151), .08 for female graduates (N=235), and .26 for females without diplomas (N=491). The effect on number of arrests of having a diploma was significant across sites; in each site subjects with diplomas had fewer arrests than those without diplomas. Of course, there is nothing in the data to suggest that having a diploma causes one to be arrested less often (or that being arrested less often causes one to get a diploma). Furthermore, there was no interaction between having a diploma and treatment.

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* $F(1,2159)=5.31$; $p=.02$

** $F(1,2122)=7.30$; $p=.007$

Having a high school diploma appears to be beneficial in terms of both employment and arrest variables. Being in the AYES program also seems to enhance of this effect on percent of time working. Given these results, it might be useful to reconsider some of the analyses presented in Chapter V. That is, the results discussed in this chapter either provide some (weak) support for human capital theory or are evidence that credentials are important, even in this population.

CHAPTER VIII: CONCLUSIONS

The AYES program provided 1082 high risk youths in three cities with employment training in the form of work experience and/or classroom training. The program was designed to increase participants' employability and earnings, and, thereby to decrease their subsequent involvement with the juvenile and criminal justice systems. The data presented in the preceding chapters indicate that these goals were met to some extent. The issues and the data are too complex, however, to leave without discussion of the results and their implications.

As was described in Chapter II, the AYES project consisted of a demonstration program and associated research. Applicants to the AYES program were randomly assigned to participate in the program, and thus became members of the experimental group, or were denied program services and became part of the control group. Data were collected on all research subjects, experimentals and controls, at the time of their application to the program, at program exit (or six months after intake for controls), and were followed up for eight months subsequent to exit. At each interview, data were collected on the subject's education, training, and employment experiences during the period since the last interview. These data were compiled into a unified database and used to assess differences between experimentals and controls which might be attributed to treatment effects of the AYES program.

The data analyses were complex and extensive, and detailed results of them are presented in the preceding chapters. The most consistent and reliable finding was that experimentals were more likely than were control group members to have obtained employment at some time during the follow-up period; 51% of the experimental group as compared to 41% of the control group were employed during this period. Similarly, experimentals had higher mean weekly earnings ($63.16) than did controls ($47.67), and worked significantly more, averaging 26.2% of the follow-up period, than controls, who averaged 19.2% of the period working. These effects are important indicators of program success, and imply that the AYES program succeeded in its goal of improving employability and increasing earnings. It is also clear that the program's effects on earnings and percent of time worked flow from the fact that more experimentals secured jobs, rather than from experimentals securing better jobs than controls. Analyses of the working subsamples of each group showed that working experimentals earned no more than working controls, and that experimentals and controls held their jobs for equal lengths of time.

The program's hopes for improving the quality of employment for participants seem somewhat naive in retrospect. The target population faces structural barriers to employment based on its youth, its distinct lack of human capital, its involvement with the criminal justice system and its racial/ethnic minority status. Those are not barriers that are likely to be overcome by a mere six months of work experience and training, even if the subjects actually participated for that long a period. They did not, of course. Thus, helping participants to secure more, if not better, work could be considered a notable accomplishment despite the fact that it falls somewhat short of the original objectives.

An investigation into the reasons for the greater success of experimentals than controls in obtaining employment is important. It seems likely that the general effect on employment emerged from a complex interaction of program effects, attributes of the participants prior to program entry, and post-employment opportunities available to members of this population. Each of these elements varied by site, as did the magnitude of the difference between the experimental and control group. Albuquerque had the highest percentage of employed subjects, 64% of the experimentals and 55% of the controls. However, the largest difference between the two groups was in New York--44% of the experimentals and 27% of the controls worked a difference of 17%. The Miami site showed intermediate rates of employment and the smallest difference between the two groups--47% of the experimentals and 42% of the controls were employed at some time during the follow-up period.

The improved levels of employment cannot be attributed to attitude changes brought about by the program. While the SAS scales were designed to measure changes in work-related attitudes and knowledge among the program participants, the research indicated that no such changes were brought about.

Nor can the general effect on employment be explained in terms of the job development and placement function of the programs. As indicated in the process analysis, this component of the program was not adequately implemented in Albuquerque or New York. Moreover, in Miami, which carried out an active job development component almost from the beginning of the program, the difference between the percent of experimentals employed and that of controls was only five percent -- the lowest of the three sites.

From these considerations we infer that improved employment of experimentals resulted from the program's creating an environment and network that assisted participants in finding work. Many of the participants were unconnected with job search networks before coming into the program. In that state of isolation, change in the person's employment situation was unlikely. The AYES program brought some of these individuals together in a situation whose raison d'etre was securing jobs; it brought them into contact with people who encouraged them to seek work, or at least accept an opportunity if it came along. In this way, AYES seems to have affected the employment of its participants, despite the absence of an aggressive job development component. Thus, the program's environment provided the participants with a job-seeking network, rather than the identification of specific jobs. Of course, an active and effective job development unit would almost certainly have enhanced the effect.

This effect varied by site because it was mediated by contextual factors (e.g., availability of opportunity) and personal attributes of the subjects. It is likely that the effect was least evident in Miami because of the interplay between unfavorable economic conditions and lower levels of education and English literacy among the research subjects. The Miami AYES program had a sizeable number of recent Haitian and Cuban refugees, many of whom could not read or speak English. This would be a major disability, especially for the Haitians who spoke only Creole or French, and could well negate the positive effects of the Miami program's job development effort.

Education (as an indication of pre-program preparation) might also explain the relatively high rate of employment of both experimentals and controls in Albuquerque; 43% of the Albuquerque AYES research subjects had diplomas at intake. Analysis of follow-up data revealed that treatment and having a diploma had an interaction effect on earnings. Experimentals with high school diplomas were more likely to have worked than experimentals without high school diplomas, but this was not true among the controls. Since Albuquerque was the site with the greatest proportion of high school graduates, we would expect this effect to be felt most strongly there.

The difference in employment between experimentals (44%) and controls (27%) was greatest in New York. This may be attributed to the devastating effect of isolation in a city as large as New York. That is, in a city the size of Albuquerque (with a population of fewer than 300,000), it is likely that even the population served by the AYES program would have some job-seeking network. In contrast, many of the controls in New York might have very few affiliations with working people and little or no access to a job search network. Entry into AYES may have provided some necessary role models and contacts to increase the participants' motivation to look for employment or to accept opportunities when available.

Considering the state of the local economies and the attributes of the target population, could the AYES program have exerted a more powerful impact on the post-program employment of participants? Surely the program had no capacity to effect change in the local economy. But the data do suggest that the general employment effect might have been magnified if the participants had been given more treatment and if the job development and placement components had been more effectively implemented.

The limited amount of treatment given was a function of both the duration of the program and various deficiencies in program implementation. From the beginning, program operators, voiced concern that six months of service were not sufficient for members of such a severely disadvantaged population. The typical AYES participant was 18.8 years old, had 10.4 years of education, was Black or Hispanic, and had little or no work experience. Six months of employment training is not enough for such individuals to experience a significant marginal increase in their stock of human capital. For example, very few participants received their GED while in the program (see Appendix A). According to program staff, this was largely due to their needing basic education before they would be prepared to embark on a GED preparation course. But providing both effective remedial education and GED preparation in six months is virtually impossible. In this regard it is important to note the significant interaction between treatment and having a diploma at intake. Those experimentals who came into the AYES program with a high school diploma were significantly more likely to obtain employment than those who did not have a diploma at intake.

While the material presented in the process analysis (Appendix A) indicates that there were differences among the sites in their implementation of the various program components, it is also clear that in none of the sites was the program implemented as fully and effectively as had been hoped. The many reasons for implementation deficiencies, from the budget uncertainties and budget cuts through the inadequate provisions for winding down program operations, are detailed in the process analysis. Those areas in which the implementation difficulties seriously affected service delivery included: (1) inadequate Model II placements in New York, (2) major problems in implementation of Model III in all three sites, (3) lack of job development services for most of the program in Albuquerque and New York, and (4) demoralizing effects of the limited period of program implementation. Each of these represented a failure to implement the AYES program as planned and resulted in diminishing the services given to participants.

Insufficient treatment also resulted from individuals leaving before completing the full 26 weeks of program participation. Whatever the reason for early termination, it reduced the amount of treatment experienced by experimentals, since once an individual was assigned to the experimental group and began program participation, he/she was counted as an experimental. Thus, all data analyses comparing experimentals to controls used a very heterogeneous experimental group, containing some individuals who had received the full 26 weeks of AYES services and others who might have received as little as one hour of service. As discussed in Chapter VII, there was evidence indicating that the more time spent in the program, the greater percent of the follow-up period a participant was likely to work. Therefore, to the extent that participants could be kept in the program for larger periods, the size of the program effect might have been increased.

In addition to program factors, economic conditions surely suppressed the effect of the AYES program. The first AYES participants started the program in August 1980 and the last of the eight month follow-ups were completed in May 1982. Throughout this period unemployment rates, especially for youth, were high and worsening. The competition for low level jobs was therefore very stiff, and the marginal improvements in skills that might have been obtained by AYES youth might not have been sufficient to overcome the worsening economic conditions.

In addition to the employment outcomes, the AYES research focused on criminal justice outcomes. The analyses of official record data did not reveal any differences between experimentals and controls on post-program arrests or convictions. While there was a relationship between number of hours spent in the program and number of post-program arrests, it is not clear whether this is a program effect or simply a function of self-selection. That is, there may well have been some factor, outside the program, which determined both who stayed in the program and who got arrested.

Given what we now know about the program and the research population, the lack of effect on arrests is not surprising. Only about a third of the AYES research subjects had a record of arrest in the two years prior to intake. Since the research found the number of prior arrests to be best predictor of post-intake arrests, the volume of such arrests was likely to be small in a 14 month period, even in the absence of the program. It is clear from the data that the AYES sample was not drawn from a "hard core" offender population, despite recruitment from criminal justice agencies. In view of that fact, the AYES program would have had to exert an extraordinarily powerful effect on participants to show a statistically significant difference in arrests between experimentals and controls. Thus, the lack of effect in this area may be more indicative of a failure to recruit a very criminally active population of subjects than of a failure of treatment.

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APPENDIX A

A Description Of Program Implementation In The Alternative Youth Employment Strategies Project

INTRODUCTION

The Alternative Youth Employment Strategies (AYES) Project was a research and demonstration project designed by the Vera Institute of Justice in conjunction with the Office of Youth Programs (OYP) of the Department of Labor (DOL). The purpose of this project was to add to the body of knowledge about youth employment problems and to evaluate the effects of an employment training program on those problems. AYES was one of forty demonstration projects funded by OYP and aimed at a variety of populations. The target population of the AYES project was 16-21 year old "high risk youth" who were CETA-eligible and not currently employed or in school. "High risk youth" was defined by Vera as "youth who have had, or show a substantial likelihood of having, involvement with the juvenile or criminal justice system" (Grant Plan, 1980:p.1).

The basic design of the demonstration project was formulated by Vera and set forth in a proposal submitted to OYP in 1979. DOL awarded a planning grant to Vera in October 1979. Under the terms of the grant, Vera served as the Central Research Agent (CRA) for the project thereby assuming responsibility for designing the program and models, overseeing and assisting local agencies in program implementation, designing the research, collecting and analyzing the data, preparing interim and final reports and managing all financial transactions pertaining to the planning, implementation and evaluation of the project. Working with OYP staff, Vera further specified the program and research designs during the first half of 1980. In the summer of that year, Vera negotiated Grant Plans with the prime sponsors and implementing agencies in each of the three program sites selected -- New York, New York; Miami, Florida; and Albuquerque, New Mexico. The Grant Plans set forth the specific structure, staffing and operations for the conduct of the program in each site. Program implementation actually began in July, 1980 and support for program operations terminated on September 30, 1981.

The programs were implemented by local service agencies in each site. In New York, the Court Employment Project (CEP) implemented the project after being selected by the CETA prime sponsor, the City's Department of Employment (DOE). CEP is a non-profit agency which has, since the late 1960s, provided court diversion and employment-related services to people brought before the criminal courts of the City. At the time CEP was selected to implement the AYES project, the agency was operating a program providing alternatives to incarceration for people awaiting sentence in the Supreme Court. In addition, CEP had operated summer youth employment programs for several years, as well as a YETP classroom training and work experience program, and still had some young people under supervision who had been diverted from criminal adjudication to the program.

Prior to taking on the AYES program, CEP operated essentially on City funds from the Human Resources Administration. AYES was taken on as a separate, virtually self-contained program. It had its own staff and administrative structure, and, in fact, took many of its criminal justice-referred clients from other CEP programs. The AYES Project Director reported to the Associate Director of CEP, and the agency's fiscal department handled AYES financial affairs.

Finally, it should be noted that CEP had a fairly extensive prior relationship with the Vera Institute of Justice. Vera designed the Court Employment Program in 1967 as a large scale criminal court diversion project, funded primarily by the U.S. Department of Labor. CEP became a separate, non-profit corporation in 1971 and subsequently operated with City funds. From 1976 through 1979, Vera conducted an extensive evaluation, using an experimental design, of CEP's court diversion operations. The study was funded by the National Institute of Law Enforcement and Criminal Justice (now, the National Institute of Justice) and resulted in an NIJ publication in 1981 entitled, Diversion of Felony Arrests: An Experiment in Pretrail Intervention, by Sally Hillsman Baker and Susan Sadd.

In Miami, the program was implemented by the prime sponsor, the South Florida Employment and Training Consortium (SFETC). That agency served as the CETA prime sponsor, and general employment planning agency for five municipalities -- the City of Miami, the City of Miami Beach, the City of Hialeah, Monroe County and Dade County. AYES was set up as a program of the Consortium located in separate quarters in the Liberty City section of Miami. The project had its own staff and director who reported to the Executive Director of SFETC. The prime sponsor was responsible to Vera for the project financial operations.

The project also enjoyed the very active support of the State Attorney's Office. In fact, the Chief Assistant Administrator of that office was assigned to work closely with the SFETC Director and the AYES Project Director throughout the life of the project. This arrangement greatly facilitated the project's efforts to encourage referrals from criminal justice agencies and to establish working relationships with other training and service agencies in the case.

In Albuquerque, the project was implemented by the prime sponsor, the City's Office of Comprehensive Employment and Training Administration (OCETA). Although the agency did not normally assume direct responsibility for program implementation, it did so in this case because of the size and complexity of the program. As in Miami, AYES was located in separate quarters and had its own staff and director. The latter person was hired by OCETA and reported directly to the Deputy Director of that agency.

In all cases, the implementing agencies operated on a subcontract from Vera and were responsible for implementing the program and managing their finances in accordance with procedures and guidelines specified by Vera. They were also responsible for cooperating fully with the demands of the research.

To carry out its multiple responsibilities, Vera central staff was divided into three parts. All financial matters pertaining to the programs and the research work were managed by the Institute's Fiscal Department, under the director of the corporation's Treasurer. Providing technical assistance and oversight relating to all phases of the programs in the sites was the responsibility of two Program Officers working under the direction of an Associate Director of the Institute. A staff of researchers, under the direction of the Projects Research Director who, in turn, reported to another Vera Associate Director, was responsible for all phases of the research, from design to final report; local research personnel were employed at each site to randomly assign program candidates to the experimental or control group and to help determine the placement of experimentals into one of the program models. In addition, the site researchers administered all the research instruments to experimentals and control subjects, collected data on their criminal justice involvement, and transmitted all data to Vera Central. The site researchers were responsible to the Project's Research Director.

At each site there were three program models designed to improve the employability, job holding skills, and post-program placement opportunities of participating youth. The three models were: Model I, work experience; Model II, vocational training or education; and Model III, a combination of work experience and education or vocational training. Table 1 presents the numbers of participants in each model across all three sites.

TABLE 1

Number of Program Participants in Each Model, by Site

	New York	Miami	Albuquerque
Model I	139	127	91
Model II	139	113	103
Model III	133	136	101
TOTAL	411	376	295

In addition to educational, vocational training, and work experience activities, program participants were to receive counseling and job placement services. Participants were paid the standard minimum hourly wage for time spent in the program. The maximum length of program participation was 26 weeks. All participants who completed the full 26 weeks in the program or whc left the program for school, another training program, or work were classified as "positive" program terminations. All participants who left the program for disciplinary reasons or who refused to continue program participation (but did not leave AYES for employment or school) were classified as "negative" program terminations.

This appendix to the research report describes in detail the nature of the programs operating in the three sites, including the problems encountered in implementation, how program operators attempted to resolve those problems, and the site staff's perception of the strengths and weaknesses of their program. The information in this process analysis report was obtained from several sources including: in-depth interviews with AYES program and research staffs at the sites, Vera personnel, and CEP staff working on projects other than AYES; reviews of participant files; research data; program reports submitted to Vera; and memoranda and documents written by Vera and AYES staff throughout the course of the project.

It will be noted that the report on New York is more detailed than the site reports on Miami and Albuquerque. The uneven length reflects certain facts about the manner in which the process analysis was conducted, rather than a greater prevalence of implementation problems in New York. The New York report was done first. This meant that various dimensions of the program were described in considerable detail in New York so they could be handled more summarily in the other site reports. This also meant that the authors spent more time analyzing and describing the problems of implementation in New York. Finally, the New York site was physically close to Vera and, therefore, program operations and program staff were more readily accessible for Vera research staff to observe and interview. In fact, while writing the summary of program operations in the New York site, the research staff were able to review program records and participant files; the opportunity to do this was much more limited in the other sites.

The purpose of this process analysis is to provide the reader with a reasonably detailed description of the services offered in the three sites, the manner in which they were offered and some of the factors that may have limited the potential effectiveness of those services. Such descriptions should be useful to other program operators who might wish to implement similar programs without falling prey to all the pitfalls encountered by those who went first. More importantly, however, the report describes more precisely the nature of the project's independent variables. Thus, if it is hypothesized that certain effects will flow from a program offering vocational training and job placement, the researcher must assess the extent to which the training and placement services were actually offered and received by the participants. In chapters 2, 3, and 4, this report provides specific information of that kind separately for each of the sites. We have used the descriptions presented in these chapters to interpret some of the outcome data that are presented and analyzed throughout the research report.

Nothing is ever as clear and clean in practice as it is on paper. Thus, program implementation typically fails to enliven all the dimensions of program design. The ways in which and the extent to which this was true of the AYES program varied with the site and the program model involved. While the deviations from program design are detailed for each site in the chapters that follow, there are a few observations which form a general context within which each site report should be considered. These observations are as follows:

1. The AYES program was large and complex, involving 300 to 400 individuals in each site participating in three rather different program models; it also required complicated screening and assignment procedures that necessitated the processing through intake of two to three times the number of program participants. In each of the three sites, staff members at the prime sponsor agency expressed concern about the size and complexity of the program. This factor alone suggests the need for an extended planning period at each site. As indicated in the next chapter, however, the planning period was too short and somewhat uncertain at all of the sites. One result of this inadequate planning was that the intake process took a good deal longer than expected and thus hampered the timely implementation of program operations for the first wave of participants.

2. Insufficient planning is endemic, of course, in social programming. Often it is possible to make compensating adjustments during the early months of implementation. In the case of the AYES program, however, there was little time for such adjustments. The entire group of participants in each site had to be introduced into the programs, provided service and processed out of the program within 12 to 14 months from the beginning of intake. No provision was made for continuation of the program beyond the end of the data collection period. This fact has several negative consequences for a demonstration project.

In the first place, the research period began with the first day of program intake, thereby providing no time for the program staff to make adjustments and stabilize operations. It is certainly true, therefore, that those admitted to the research sample during the first couple of months of program operation did not experience the same quality of services during their early weeks of participation as did those admitted later. In effect, when evaluative research begins with the first day of the program, the program is likely to be penalized by a period of uncertainty and adjustment that is inevitable.

Second, the implementation schedule required that intake and program services be conducted simultaneously. Since implementation funding was always tight and often uncertain, the two functions had to be performed essentially by the same staff members. This strain was exacerbated by the length and complexity of the intake process. Something had to suffer and, in most instances, it was the frequency and intensity of counseling and job preparation. Moreover, in a program requiring so many research subjects (both participants and controls), the need to conduct both intake and service delivery at the same time helped to create a certain tense atmosphere during the early phase of the program.

Third, the limited operational period had a particularly negative effect on the implementation of Model III. Participants in that model were to experience a mixture of education or training along with work experience of some sort. It was hoped that there would be a complementary relationship between the training and the work experience and that the participant would experience both within essentially the same time period. The program design called for a half-day at each or, failing that arrangement, alternate periods of training and work experience with no single period exceeding two weeks.

If the programs could have operated for longer than the 12 to 14 months provided, and if the program operators had not been required to fill all three program models at approximately the same rate, Model III might have been implemented more effectively. These conditions would have permitted the scheduling of participant intake to coincide more closely with the educational and training cycles followed by other agencies in the sites. Moreover, the program counselors would have had more time to arrange complementary training and work experience placements and reasonably short alternate periods of participation in each.

In fact, however, intake had to be completed as quickly as possible and the models had to be filled at approximately the same rate. As a result, efforts to find educational or training slots were being made well after particular training cycles had begun. A complementary relationship between training and work experience was the exception in all sites and often a coincidence, when it did occur. It is likely that many participants, especially those in New York and Albuquerque who left the program after several weeks, received uneven amounts of training and work experience. The net result of all of this was that the features that distinguished Model III from the other two models in program design were not fullyrealized in program implementation.

The limited period of program implementation had at least one other negative effect on the program. Since no provision was made for assuring program staff of employment beyond the end of the data collection period, they became anxious about and began searching for future employment months before the programs actually ended. In some instances, this resulted in the premature leaving of an important and effective staff member. In all instances, it heightened the level of anxiety and lowered morale among program staff. Moreover, there is some suggestion that an atmosphere of imminent doom developed in the last couple of months. If so, this may have adversely affected both the quality of service offered by the staff and the enthusiasm of the participants' response during that time.

3. As is detailed in the next chapter of this report, the budget commitment of the Department of Labor to this demonstration was uncertain throughout the life of the project. This had a negative impact on the substance of the program and the morale of the program staff in all sites. When the implementation budgets were finally approved, they were for less money than was anticipated during the planning period. Thus, not all of the positions which were originally thought necessary could be created. Moreover, operating budgets were reduced during the life of the programs. As a result, some positions were filled later than was de-

sirable (job developer positions, for example) and program directors never had a clear understanding of the resources that would be available to them.

Skimping on the job development and placement functions is obviously counterproductive in a program of this kind. However, because the program was of limited duration and uncertain resources, and because so many other components of the program had to be organized before the job development and placement service, it is not particularly surprising that this function bore the brunt of the program's budgetary problems.

There is one final point to be made by way of introduction to the process analysis. People experienced in service delivery and program management know that the personal commitment and skill of staff and the quality of leadership exercised by administrators are crucial ingredients in the success of a program. Yet researchers conducting process analyses rarely address these variables. We are no exception to that rule. The reader should be assured, however, that our failure to assess staff commitment and the quality of administration reflects our inability to measure and analyze these qualities in a useful way, rather than a judgment about their importance.

Some of the conditions described in the chapters that follow created implementation problems for all of the sites, while others were peculiar to one site. No attempt has been made to compare the sites with respect to the overall level of difficulty they faced in implementation or the quality of leadership they enjoyed during the life of the program. We do have a general impression, however, that overall the implementation of the program conformed more closely to the program design in Miami than in either of the other sites. It seems reasonable to assume that this resulted, at least in part, from the very active support of the prime sponsoring agency and the State Attorney's Office and the very competent and charismatic leadership exercised by the Project Director during the early months of the program.

CHAPTER ONE: PLANNING AND BUDGET

A. Planning and Design

In 1979 the Office of Youth Programs (OYP) of the Department of Labor (DOL) was funding a large number of youth employment programs as part of its Knowledge Development Plan. Since OYP was interested in funding a project with a target population of "high risk youth," in the spring of 1979 DOL approached Vera about the possibility of Vera acting as the Central Research Agent (CRA) for a research demonstration project serving this population.

At DOL's request, the Vera Institute submitted a proposal for a four site operation with 450 participants (and 450 controls) per site. The DOL responded favorably to the proposal, but requested modifications and further specifications of the budget. The proposal went through two revisions and was finally submitted to DOL on July 18, 1979 at an estimated cost of $8.25 million for program operations in four sites, plus $1.5 - 2.0 million for the CRA. After negotiation, DOL awarded Vera a planning grant for the period October 1, 1979 through March 31, 1980. During this period Vera expected to articulate the research and program designs, select the sites, negotiate contracts with the sponsoring agencies at those sites, and begin to hire senior-level program staff. At the time the planning grant was awarded, Vera requested that the Department of Labor react to the program description in the proposal and approve the full project by January of 1980.

In its initial discussions with DOL, Vera suggested that DOL grant funding for 18 months of program operations. Vera asserted that a new project requires at least two months of existence before it can attain an efficient level of operations. Second, Vera claimed that during the last two months of operations a program is negatively affected by staff attrition and diminished staff morale.

Vera claimed that for AYES to have continuity and provide a realistic period for program evaluation, 18 months of operation would be necessary. However, DOL authorized funding for only 13 months of program operations, but stated that they would attempt to arrange for the institutionalization of the program after the expiration of the grant. This would have mitigated the effect of wind-down on program operations. Unfortunately, DOL never arranged for the continuation of the program after the expiration of the grant. Program operators in New York and Miami were able to locate (without the help of OYP) some additional sources of funding for continued operations, thereby providing some continuity for program operations. In Albuquerque, program operations ceased after expiration of the grant. During the planning period a number of significant modifications were made to the program design. After discussion, DOL decided in January to limit the number of sites to three; New York, Albuquerque, and Miami were approved as the three sites. The sponsoring agents selected were the Court Employment Project (CEP) in New York, the South Florida Employment and Training Consortium (SFETC) in Miami, and OCETA in Albuquerque.

At this time DOL also introduced the idea of random assignment to model. In Vera's proposal to DOL, assignment of experimentals to program models was to be done using "guided choice" interviews. This would allow the participant to express his/her desires regarding program participation, but would allow the program staff to guide the participant, based on his/her aptitudes, skills, and needs. Random assignment to model was suggested by DOL as the means of testing which model was most effective in terms of fewest dropouts and other impact measures (e.g., post-program earnings, recidivism).

Vera argued against the idea of random assignment to model on the basis that it made no sense programmatically and would increase the number of early dropouts (thereby decreasing exposure to the program). Furthermore, DOL's proposal involved "guided choice" for the first phase of intake and random assignment for experimentals taken in during the second phase. Vera Research staff believed this would severely limit inferences that could be drawn from research outcomes. AYES program staff were dissatisfied with this requirement and predicted it would impact negatively on the program's ability to deliver services to its clients.

DOL did not approve the full grant in January of 1980, but did award Vera a no-cost extension of the planning grant until June 30, 1980. Throughout the planning phase of the project, Vera continued to conduct budget negotiations with DOL and with the proposed sponsoring agencies at the sites.

From Vera's point of view, the ideal process for negotiating approval of the budget would have been for DOL to react to Vera's program and research designs First, and let those substantive considerations determine budget amounts. Alternatively, DOL could have provided Vera with a budget ceiling and specifications of other program restrictions. However, DOL followed neither course, and Vera was unable to obtain timely approval of the budget and program design. This situation was exacerbated by the constant staff turnover within DOL. Between October 1979 and June 1980, Vera had three different DOL program Officers.

Budget negotiations continued throughout the planning period and into July. On July 18, just prior to the first day of intake, DOL approved funding for program and research operations until September 30, 1980 based on an estimated cost of $1,770,000 per site for the entire project. For the duration of the project DOL approved AYES funding in increments of three to nine months. This process forced Vera Program Officers and Research staff to spend an inordinate amount of time calculating and re-calculating budgets for the three sites and the CRA. In addition, DOL generally did not approve the latest increment until the previous period had already expired. Finally, the replacement of high-level DOL staff following the 1980 elections resulted in additional delays and a severe reduction in funding.

In April 1981 DOL approved another increment in the AYES budget, this time through December 31, 1981. This extension also involved a substantial budget cut, to approximately $1,573,000 per site. The collection of 8-month follow-up interviews had been scheduled to continue through May 1982, but Vera now had no assurance that there would be any research operations after the end of 1981. The quandary facing Vera was whether or not to initiate work on the 8-month follow-up before receiving notification that the grant would be extended. Vera chose to implement the 8-month follow-up, and, fortunately, DOL granted, in December, a no-cost extension through September 30, 1982; in September they granted another no-cost extension through December 31, 1982. This uncertainty about funding greatly complicated the research effort.

B. The Structure of AYES: Vera and Local Program Operators

As the Central Research Agent (CRA) under contract to DOL, the Vera Institute was responsible for program design; program implementation and technical assistance; program monitoring; research design development; research instrument development; data collection; monitoring program compliance with the research design; construction, maintenance and analysis of the research database; and report writing.

To carry out these responsibilities, Vera established a central staff consisting of Program Officers, a Project Research Director and research staff, and clerical support staff. In addition Vera hired a Research Associate in each of the three sites and assisted that person in hiring part-time assistants and interviewers. Vera Central Staff trained the local researchers and monitored the research process. Local Research Associates were responsible for the random assignment of eligible applicants to the experimental and control groups; ensuring that research instruments were completed for all research subjects; developing subject tracking systems and maintaining contact with research subjects; conducting follow-up instruments; collecting juvenile and criminal justice system data; and transmitting data to Vera Central research.

Once the program had been designed, primary responsibility for operations rested in the sites with Vera providing technical assistance through its two Program Officers. In Albuquerque the program was operated by the Office of Comprehensive Employment and Training (OCETA); AYES staff were employees of the City of Albuquerque and were hired through the City personnel system. The program was located in an OCETA-owned building. In Miami the program was operated by the South Florida Employment and Training Consortium (SFETC), AYES staff were employees of the City of Miami. The Miami AYES project was housed in a rented building in the Liberty City area of Miami. The New York program differed from those in Albuquerque and Miami in that it was operated by a private agency, CEP; thus AYES staff were employees of CEP. The program was located within the CEP offices.

With minor variations, the organization of the staff of the three AYES programs was the same. Each site had a Project Director, a Field Operations Director, a Director of Education and Training (E&T), Deputy Directors of E&T and Field Operations, vocational counselors, work site supervisors, and field representatives. The Miami and New York AYES projects also had job developers on their staffs. In Albuquerque, program operators subcontracted Job Placement Services with another agency. The local staffs were hired by the sponsoring agencies with assistance from Vera Program Officers.

Also present at each site were the local research staff; in Miami and New York, the research staff were employees of the Vera Institute. In Albuquerque, the research was subcontracted to the Albuquerque Urban Observatory (of the University of New Mexico), but continued to be monitored by the Vera Institute to ensure its compliance with the research design. Having the local reseachers employed by Vera rather than by the local AYES projects was a conscious attempt to maintain the integrity of the experimental design. Thus, the local research staffs were supported on Vera Central budgets, but were housed in AYES offices. Furthermore, while AYES program staff reported to the local Project Directors (who were monitored by Vera Program Officers), local research staff reported to the AYES Research Director at Vera.

C. The Effects of Uncertainty On Project Implementation

The budget cuts and uncertainty about those cuts had a profound impact on Vera's capacity to plan the program; consequently, the relations between Vera and program operators were affected. Vera had expected to provide local program operators with a program manual before implementation began. The manual was to specify systems, forms, procedures, and personnel requirements; Vera expected to negotiate modification of that manual pursuant to dicussions with the program operators. Since, during the planning stage, DOL provided Vera with neither bottom-line budget figures nor feedback on the program design, Vera's Program Officers were forced to plan and implement the program simultaneously, even while revising the budgets for DOL approval. Instead of providing a program manual in advance of implementation, Vera was forced to develop a series of over 20 Field Memoranda, each requiring program innovations while both intake and program operations were well underway. Not surprisingly, the lack of time to test systems and the consequent succession of Field Memos generated some confusion and annoyance towards Vera as well as uncertainty about how the programs were expected to operate. For example, as a result of DOL's failure to provide timely direction and approval of budgets, Vera was placed in the unenviable position of requesting that local agencies hire and train staff and start intake without official authorization of funds from DOL. For their part, the agencies were reluctant to accede to this request, and, for example, the hiring of site staff was delayed until just prior to intake. In general, this pattern of extending the project by increments, and waiting until the last minute to do so, weakened Vera's credibility as a provider of helpful assistance to the sites.

The lack of an adequate planning period, combined with budget uncertainty, had several negative effects on the implementation of AYES. Several programmatic and research issues should have been worked out with the sponsoring agents at each site before the program began. These issues included: hiring program staff, establishing outreach strategies, planning intake procedures, establishing priorities for providing of services to program participants, determining disciplinary procedures, instituting forms and outlines for reports on program and research operations, and locating placements at outside agencies for program participants. During this period, Vera was able to arrange plans for these aspects of program implementation, but the level of planning could not be as complete as desired. While details about the effect of the planning and budget complications on these areas of program operations are contained in later chapters, a few salient issues are addressed below.

The reduction in funds limited the number of line-staff working on the project in every site (especially worksite supervisors, vocational counselors, and job developers), the secretarial staff needed for bookkeeping, and the number of participants allowed into the program. Uncertainty about funding and the increased caseloads of AYES staff caused by budget cuts reduced the volume of service provided to participants and appeared to have lowered staff morale at the sites.

The AYES project was structurally very complicated, both in the original plan and in the reality of the program. Without adequate time for planning, these inherent complexities led to problems in communication between Vera and the sites. Each new directive from DOL (e.g., random assignment) had to be transmitted from Vera to the sites; within Vera there were three components, program, research, and fiscal. At the sites there were sponsoring agencies, program staff, and Vera research staff. It was sometimes unclear to site personnel whether a particular specification originated in Vera Program, Vera Research, or DOL. As a result of this confusion and because of Vera's role as enforcer of sometimes unpopular rules, relationships between the sites and Vera were sometimes strained. Aspects of the program design most often misunderstood and/or criticized by program operators included the Adkins and VITAS systems for vocational assessment and counseling, Vera's heavy emphasis on vocational rather than personal counseling, the discipline/termination system, and Vera's requirements of detailed monthly reports on program operations. Each of these dimensions of the program are discussed in more detail in the pages that follow.

Another source of conflict (which might have been alleviated had there been more time for planning) was the role Vera played in hiring AYES personnel. Site staff felt that Vera was overly involved, while Vera Program Officers countered that they should have been more involved. While the problems varied from site to site, in each there was some uneasiness over Vera's role in hiring. These conflicts, more pronounced in New York and Albuquerque than in Miami, were seen by some as having had a negative effect on the working relationship between Vera and the site staffs; moreover, these problems became a drain on the time and energy of senior personnel involved in the AYES project.

CHAPTER TWO: NEW YORK PROGRAM OPERATIONS

A. INTAKE

Intake was originally expected to occur in two 4-6 week periods, with each period attracting 225 experimentals and an approximately equal number of controls. (Since a certain number of assigned experimentals were expected to drop out prior to program participation, the control group was expected to be slightly larger than the experimental group.) According to the Grant Plan, the first phase of intake was to employ a guided choice assignment to model, while all program terminations during this period were to be replaced by experimentals randomly assigned to model. The grant plan stipulated guided choice assignment to model during phase two of intake. Just prior to the start of intake, however, this plan was revised. In order to ensure sufficient numbers of active participants throughout the course of the project, a plan for continuous intake was developed. Instead of having two distinct phases of intake, all experimentals terminating prior to six months of program participation would be replaced immediately by random assignment; all experimentals completing six months in AYES would be replaced upon their graduation by guided choice experimentals. It was expected that approximately half of the experimentals would complete the full six months in the program and that this plan of intake would generate roughly equal numbers of guided choice and random assignment experimentals.

Unfortunately, it took almost four months to attract the first 225 AYES participants; therefore, there was no opportunity at this time to replace participants by random assignment to model. DOL, still requiring random assignment, insisted that all experimentals taken into AYES after this original 225 be randomly assigned to model. Therefore, throughout the course of this report, we will to refer to two distinct phases of intake: the first 225 (guided choice) experimentals, who were taken into the program between August 8 and

November 28, 1980, and the second group of 185 randomly assigned experimentals taken into the program between December 8, 1980 and March 26, 1981. Since program staff had both programmatic and intake functions, the unexpected duration of both intake periods caused considerable confusion and hardships for both program operations and program staff (according to interviews with program personnel).

Although the Grant Plan called for only a 50% criminal justice (CJ) referral rate for AYES, CEP staff originally expected to attract a substantially higher proportion of CJ referrals to the program. However, difficulties in attracting CJ-referrals to AYES led to a 56% CJ referral rate.

A brief description of the program population in New York is presented below in Table 2.

TABLE 2 (n=411)

Referral source	
CJ	56.6%
non-CJ	43.4%
Assignment Type	
guided choice	55.3%
random	44.7%
Sex	
male	69.5%
female	30.5%
Had Diploma?	
yes	8.5%
no	91.5%
Ethnicity	
white	2.7%
Black	67.3%
Hispanic	30.0%
Mean Age	19.0

There was some initial confusion regarding the definition of criminal justice referral and the policies guiding the use of criminal justice information by program staff. The definition of criminal justice referral was eventually clarified in a 7/16/80 memo by a Vera Associate Director to AYES staff:

> A criminal justice referral is a person referred to AYES by a criminal justice agency, or by a social service agency that originally accepted the person on referral from a criminal justice agency. A referral of the first kind is direct, while the latter type is an indirect criminal justice referral.
>
> The question of whether or not the person is currently under the authority of a criminal justice agency is not relevant to his eligibility for AYES, or his being counted as a criminal justice referral.

AYES program staff at all three sites were informed at intake whether a candidate was an indirect or direct CJ referral and whether that candidate's obligations to criminal justice agencies (e.g., a probation requirement to attend drug rehabilitation sessions) would interfere with his/her AYES participation.

Outreach

The agencies contacted for criminal justice referrals included: the courts, parole, probation, the Court Employment Project (CEP), the New York City Riker's Island Correctional Facility, and New York State Division for Youth (DFY). Because AYES/CEP staff were interested in attracting as high a percentage of CJ-referrals as possible, the director of CEP at that time decided to refrain from contacting other agencies whose candidates would include a high percentage of non-CJ referrals. Brochures and pamphlets were sent to CJ agencies in four boroughs (Staten Island being excluded). The N.Y. Project Director spoke in person to many of the larger agencies.

During the first phase of intake, most candidates were referred by CEP, with probation being the second largest source of referrals. Since intake was slower than expected, all experimentals were assigned to model by guided choice interviews; there was no random assignment during this period.

Several explanations for the unexpectedly slow rate of intake were suggested by program staff: the Project Director was hired only one month prior to intake, too late for him to organize a more thorough outreach effort*; vocational counselors were too busy conducting intake to maintain their contacts with referral agencies; and the AYES staff waited too long before realizing they would have to overbook appointments for intake (because many referrals never showed up). Moreover, interviewed program staff mentioned that some referral agencies were not very cooperative and suggested several reasons for that attitude. These included: the delay of program start date as well as several false start dates; antipathy towards the basic experimental/control design (an antipathy exacerbated by the apparent omission of this research element from the initial contact letters to these agencies); and lack of timely feedback to these agencies about how their referrals were doing in AYES. Furthermore, many program candidates, especially CJ-referrals, lacked adequate CETA-eligibility documents when they first appeared for intake, and, therefore, could not be accepted for program participation. Finally, a former CEP staff member criticized the structural divisions between CEP and AYES as preventing CEP staff from lending their full assistance and expertise to the outreach effort.

Trying to speed up the rate of intake, the program staff introduced several innovations during phase two of AYES program operations including: overbooking appointments in a systematic manner, often scheduling more than twice the number of appointments that were expected to appear for intake; making more extensive use of Testing, Assessments, and Placement (TAP) Centers, whose referrals it was assumed, would all be CETA-eligible; and contacting more community groups.

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* The delay in hiring a Project Director was attributable, in part, to DOL's delay in approving the project budget and, in part, to initial differences between Vera and CEP over the qualities needed in the AYES Director position.

Although these community groups were requested to refer mostly CJ referrals, in general they did not comply with that request. It was further noted that many community groups had major reservations about AYES including: dislike of the basic control/experimental design and reticence about being involved with a program located in a court employment project. During phase two intake, most referrals were made from CEP, TAP Centers, and city Probation, in that order.

In sum, most members of the AYES staff felt that candidates taken in during phase one were different from the later candidates. They believed the former were more likely to be males and criminal justice referrals, to have more extensive criminal histories, to have lower functional levels and to have entered the program when it was not at peak efficiency. Therefore, they felt that phase two participants would do better (as measured by post program employment and criminal justice involvement) than phase one participants.

Vera staff investigated these perceptions by analyzing data from the Vera questionnaire. Although there were significant differences between CJ and non-CJ referrals, there were no significant differences between phase one and phase two participants on age, sex, work history (indicated by whether the person had ever worked, and if so, whether the job was in the primary or secondary segment of the labor market), reading levels (as measured by the ETS Step test), and highest grade completed (see Table 3).

TABLE 3

COMPARISON OF PHASE ONE AND PHASE TWO

	Phase One (guided choice) n = 225	Phase Two (random assignment) n = 185
Referral Source*		
CJ	69.4%	40.4%
non-CJ	30.6%	59.6%
Sex		
male	72.3%	66.2%
female	27.7%	33.8%
Ever Worked		
yes	74.3%	75.0%
no	25.8%	25.0%
Most Recent Job Labor Market Segment		
primary	40.4%	31.8%
secondary	59.6%	68.2%
Mean Age	18.9	19.1
Mean Step Score	12.7	13.4
Mean Highest Grade Completed	10.1	10.1

* $X_2=37.5$; df=1; $p>.001$

Eligibility

To be eligible for participation in AYES, a program candidate had to document that he/she was between the ages of 16 and 21, out of school, out of work, and CETA-eligible. The original oral agreement between AYES and the New York City CETA Prime Sponsor, the Department of Employment (DOE), specified that the latter would conduct CETA-eligibility training for AYES staff. Although DOE eventually provided that training, confusion over eligibility requirements continued among AYES staff. Another problem for eligibility screeners at CEP was their lack of understanding of the distinctions between CETA- and AYES-regulations. Consequently, the process of taking in program candidates was slowed down for months before these distinctions were clarified and staff realized that certain documents could be completed at a later date.

The intake process lasted two to three days for most AYES candidates. Many candidates had to wait for hours before their eligibility could be ascertained. In response to this situation, AYES attempted to coordinate with various referral agencies a procedure for instructing their applicants on what documents would be needed by AYES; nevertheless, candidates continued to appear without their required documents.

According to program staff, many program candidates failed to complete the intake process because they were asked to leave and return at a later date with all the required documents. One staff member estimated as many as 50% of the applicants never returned for the second appointment. In addition, the criminal justice referrals, especially those applying during phase one, were more often lacking documentation for CETA-eligibility than were the other candidates; therefore, when sent home for documents, they were less likely to return for intake. Thus, the documentation requirements may have "screened out" many candidates from program participation (and thus from the research database)*. The program staff found this demoralizing as they felt that those who needed the program the most (criminal justice referrals, in particular) could not make it through intake.

III. Intake P

Pre-intake consisted of collecting basic information and establishing eligibility. After this pre-intake was completed, candidates were designated "Vera ready." In the beginning of the program, candidates were seen by research staff that same day. Later on, however, the procedure was modified: program staff, after determining the eligibility of a candidate, scheduled that person's research interviews for a later date.

When a wave of applicants (planned to be optimally between eight and twelve persons) was ready for research interviewing, a member of the Vera site research staff conducted a brief orientation. This orientation covered the experimental design (with emphasis on random selection of experimentals and controls), the confidentiality of the interviews, the basic rationale behind the research instruments, and the need for all applicants to keep in touch with the research staff. After the orientation, the Vera Research Associate picked up the CETA forms, and the Vera researchers administered the Vera research instrument. Depending on the individual's work history, the Vera interview lasted 20-40 minutes. After completing the Vera interview, a member of the research staff administered the ETS instruments to the entire group. The Vera interviews were administered individually, but the ETS instruments were given simultaneously to the entire group of candidates.

After completing the ETS interviews, a site researcher determined (using a random number table) which persons were assigned to the experimental group and which to the control group. The experimentals were notified that they were accepted into the program and that they should see a member of the program staff about an appointment for a guided choice interview. Another researcher told the controls that they were not in the program but were still part of the research. The controls were paid $10 and notified that they could speak to a member of the program staff about referrals to other programs and agencies.

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* Since eligibility screening occurred prior to the random assignment to experimental or control group, this screening could not have affected experimental/control comparisons.

Although the debriefing of controls was supposed to be handled by a member of the program staff, in actuality this task was left for the reseachers because program staff were often involved with other duties. The confusion over who was to debrief controls -- program or research -- led to some controls having to wait for an explanation of why they were not admitted into AYES. Early in the project, the controls were referred to other agencies; but eventually program staff, claiming that their referral sources were too depleted by budget cuts to be of much assistance to AYES referrals, decided not to offer a list of programs/agencies to the controls. Unfortunately, there was a lapse in communication between program and research about this change which generated some confusion for controls over whether or not they would be given a list of referral sources. These developments may have had a negative impact on the success in attracting controls for exit and follow-up interviews.

During phase one of intake, experimentals had a guided choice interview with a program counselor to determine their preferred model choice(s). These preferences were then sent to the site researcher who assigned experimentals to model each Friday. An attempt was made to offer the candidate his/her first choice (no one received worse than a second preference), while also assuring that the three models filled up evenly. Most participants were able to receive their first choice of model. For research purposes, this practice assured all guided choice experimentals an equal chance of receiving his/her model preference;

programmatically, this system enabled the program operators to avoid the potential staffing problems generated by widely divergent model sizes. In addition, each Friday the site researcher monitored the proportion of CJ referrals in the experimental and control groups.

Intake to Program Start Date

It usually took 2-3 weeks after experimental/control assignment before a client could begin his/her program participation. This time period was utilized to "acclimatize" the participant to the AYES program and accomplish various administrative tasks. The client was given a TABE (reading) test to determine functional ability. During phase one of intake, each client went through a guided choice interview with his/her counselor. In this interview the client and counselor established which model, and which slot within that model, was most suitable for that client. In addition, clients met with counselors to establish the goals of that client's AYES participation. During phase two intake, the "guided choice" interview attempted to work out for that participant the optimal program placement within the restrictions of his/her random model assignment.

Each client was expected to take a physical examination. Scheduling and conducting these examinations was sometimes difficult for program staff. Often the clients missed their appointments, which were difficult to reschedule. According to several line staff personnel, problems with scheduling and rescheduling this examination frequently delayed program start dates as much as 2-3 additional weeks; moreover, according to some program personnel it is possible that quite a few AYES clients never had their physical examinations.

Two final administrative tasks had to be completed before program participation could begin. The participant had to be placed on the payroll, and program staff often found it convenient to wait until a new payroll period began placing a participant on the payroll. Second, the client had to be placed within a program slot; program staff sometimes had to delay a program start date until that client's slot became available. According to the New York Project Director, this waiting period had some programmatic utility. The delay gave certain clients "an excuse not to show up," thereby screening out several probable early terminations from entering the program. According to most program line staff, however, this waiting period before program start date had little effect on the no-show rate.* On the other hand, the transition from guided choice to random assignment did increase the no-show rate from 2.4%(6) to 7%(13). According to most interviewed program staff, the delay generally had no effect on participant morale; Vera's database indicates that this delay had no effect on participant hours or positive termination rate.

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* A no-show is defined as an assigned experimental who never began his/her program participation. If an assigned experimental worked one hour or more, he/she was defined as a program participant.

B. FIELD WORK AND COMMUNITY RELATIONS

AYES offered two types of Model I placements: placements on AYES-supervised workcrews, where participants worked on clean-up and basic construction projects; and "single-site" placements at non-profit or government agencies, where participants were given work experience at a variety of single placement slots. Placement in one of these two types of slots depended upon the desires of the client and the availability of slots. Virtually all (95%) participants placed in workcrews were male. Overall, 72% of all Model I participants were placed on workcrews; the remainder were single-site placements.

Both workcrew and single-site placements had the same principle objective: teaching proper work habits so that the AYES graduate could be able to find and keep a job. These proper work habits included: discipline, punctuality, being able to get along with peers and supervisors, and developing elementary work skills.

While instilling proper work-related behavior was the principal goal of Model I participation, workcrew members also learned a variety of construction skills. Workcrew members began doing unskilled labor, but those who demonstrated aptitude and motivation were given the opportunity to learn one or more of the following skills: sheetrocking, simple plumbing, painting, plastering, bricklaying, hanging and setting windows, boiler installation, landscaping, and simple electrical work. Several worksite supervisors (WSS) stated that many clients had "unrealistic expectations" about what they could gain from AYES; that is, many clients expected to be able to locate highly-paid crafts positions after their tenure in AYES.

According to interviewed staff, placement on workcrews had certain advantages and disadvantages over the individual placements. On the positive side, workcrews were directly supervised by AYES staff, so AYES was not dependent on third parties for supervising and monitoring placements. In addition, workcrew members learned basic construction skills and had the opportunity to see more visible tangible evidence of their work. However, although a few workcrew members were able to locate employment through contacts established at their worksites, some staff believed that participants had better opportunities for post-program employment on single-site placements.

Some of the major worksites in New York included: housing restoration for the Banana Kelly Neighborhood Improvement Association, New York's major worksite; the Harlem Restoration Project, where workcrews renovated an abandoned building for use as offices for public service organizations; scraping, painting, and cleaning the city-owned CEP building; renovating tenant-managed apartments for the 1845-51 Tenants Association; and rehabilitating an old gymnasium at the University Settlement House.

Instructions at the worksites were usually given directly by the WSS. Occasionally journeymen hired by the contractor at the worksite gave instructions to the AYES workers. On some sites AYES workers had to "prove" to the contractors and their crews that they were responsible and competent workers. Sometimes the WSS acted as advocates for their clients, making sure that their clients were not being used merely as messengers or treated unfairly by the contractors. A few arguments arose between AYES clients and the construction workers, but these were handled effectively by field staff.

The mode of instruction was as "hands-on, trial-and error" technique. The client first watched the WSS or journeyman accomplish a task and then attempted to repeat that task under the proper supervision. Slower clients were supervised not only by the WSS but also by their more adept coworkers. If a client had difficulty learning a particular skill, he or she would be transferred to another area of skill training.

The client's ability to learn these skills, of course, varied considerably. Moreover, the field staff had somewhat different views on the relative gains of workcrew participants. It was generally conceded, however, that most clients had to overcome an initial reticence towards hard work as well as an unfamiliarity with proper work-related behavior. Nevertheless, AYES field staff generally thought that the quality of the work and the morale of the workers were good. The Field Operations Director stated that the quality of the work accomplished at the worksites improved over time. Disputes between WSS and clients, as well as among clients, was described as sporadic and minor.

The main problem facing the WSS seemed to be getting supplies and equipment from community agencies. Other problems included: gaining the acceptance of the contractors at the worksites; discipline (see later section on terminations); large case loads; and random assignment to model. According to field staff interviewed, random assignment to model precipitated many early terminations. This effect was said to be particularly pronounced at times when AYES could offer only a limited range of single-site placements, and thus many "non-construction types" had to be placed on workcrews. According to program records, however, the positive termination rate on workcrews declined only 5% from guided choice to random assignment. Intake adversely affected field work in two ways: first, field staff had to perform intake as well as field work functions for several months; second, some members of the field staff resigned over having to work at intake tasks. Finally, towards the end of the project there were sometimes problems providing a sufficient number of participants to man the workcrews at some sites.

CEP as a worksite had characteristics somewhat different from other sites. CEP was utilized as a transitional placement for many AYES clients until a single-site placement could be located for them.* Since many workers at C.E.P. were placed there temporarily, it became difficult for the WSS to establish strong working relationships with these clients.

Single-site agencies were selected according to their past experience with AYES-type populations, their willingness to be monitored, and the proximity of the agency's goals to the objectives of the AYES project. Many single-site placements were made at daycare centers, hospitals, museums, and city agencies. While AYES clients were supervised directly by agency personnel, the AYES Field Representative was responsible for screening and placing clients, verifying timesheets, offering technical assistance, monitoring the supervision by the agency, and serving as an informal counselor to the AYES participants. Clients in clerical slots received a one-week orientation by AYES staff prior to their outside placements.

The advantages perceived by program staff of being placed in a single-site placement included a greater likelihood of a transition into a job** and the opportunity to see firsthand the world of work. On the negative side, the AYES staff was dependent upon the cooperation and effectiveness of third parties to supervise and monitor their client's performance. Fortunately, there is little evidence indicating an inferior effort on the part of these agencies in New York. The positive termination rate was approximately the same for workcrew and single-site placements.

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* In-program participation data do not indicate many Model I clients with both single-site and workcrew experiences. Due to problems associated with these data, however, it is likely that more clients had both Model I experiences than indicated by the data.

** Data assessing the program participation and post-program effects of the two types of Model I experience are forthcoming. Unfortunately, termination information about post-program job placements is extremly unreliable.

The quality of work was described by staff as generally good, the clients' morale as variable. According to interviewed field staff, most clients in these placements had little or no prior work experience, poor work habits, and little understanding of the world of work; most clients were said to have made progress in work-related behavior. The most serious problem for the Field Reps was trying to get their clients to show up for work on a regular basis and on time. Random assignment was said to have had a negative impact on single-site placements because clients wanting a GED were assigned to work experience and model switches were strongly discouraged. Nevertheless, the positive termination rate at single-site placements did not decline after random assignment was implemented.

Relationships between AYES and various community groups were described as positive. In part, these amicable relationships were a function of the careful selection of cooperative placement agencies and worksites. AYES staff were also careful not to place too much temptation in front of their clients; that is, placements were not made where clients would be around drugs or large sums of cash.

There were a few minor disagreements between AYES participants and members of the communities near AYES worksites, but it appears that these problems were generally rectified promptly and expeditiously. Although most field staff stated that the community was not afraid of the AYES workers, sometimes the AYES staff had to convince community people that the project was not for "hard-core criminals." At one site, the local merchants were initially distrustful of the clients and only admitted them into their stores one at a time; this problem was soon resolved when, according to the WSS, "AYES got rid of its bad apples." The AYES clients also had to prove to the contractors and journeymen at worksites that they were capable and competent workers. There were a few minor incidents between AYES clients and the construction workers, but it appears that these problems were almost always resolved before escalating into major incidents. There were a few minor disputes between clients and court officers employed in the building where CEP was located, but the New York Project Director was credited by the former Director of CEP with doing an excellent job in settling this conflict.

Several clients obtained employment at the site of their placements. According to most field staff, many more clients would have been offered jobs had these agencies and local contractors been able to hire more workers. At some sites local merchants cashed clients' paychecks, and personnel at placement agencies often wrote letters of recommendation for AYES clients. In conclusion, relationships between AYES and the community seemed amicable, but employers located in the community did not provide jobs to many graduating AYES participants.

C. EDUCATION AND TRAINING

AYES was designed to offer both a full-time education and training (E&T) program - Model II - and a combined E&T and work experience program - Model III. In the course of planning the project, Vera, AYES managerial staff, and the sponsoring agency (CEP) expected to place most Model II and III clients in alternative schools and vocational training programs. Selection of these community agencies was to be based on their compatibility with the goals of AYES: "their (the agency's) placement opportunities..., their emphasis on individual training needs, their experience with 'high-risk' youth, and the compatibility of their entrance requirements with the vocational and educational skills of the AYES participants" (Grant Plan: pp. 12-13).

Unfortunately, this plan had to be revised when a series of problems severely impeded the efforts of AYES operators to place their clients in outside agencies. These included:

1. Due to tighter funding restrictions, CETA programs in New York had become more selective in accepting referrals. Since their training programs were held to high placement goals, many agencies had developed minimum reading and math achievement scores as entry criteria. These were typically 6th through 8th grade levels of proficiency which could be met only by a very few of the AYES participants.

2. Since AYES clients were on the AYES payroll, stipend payments to participants at these agencies would have resulted in the added administrative complications for the CETA program. It was simpler for them not to admit AYES clients.

3. Frequently there was no training program that both met the individual needs of the client and also had a convenient start-date. Moreover, because the

programs had to be completed in one year, the intake dates could not be scheduled to coincide with the cyclical start dates of the training programs.

4. While there was a multitude of programs in New York, AYES staff had no effective mechanism for evaluating their quality. Moreover the prime sponsor was unable to facilitate the placement of AYES clients into CETA educational and training agencies; as a result, AYES' credibility with these outside agencies was never clearly established.

5. There was a great deal of turnover in the E&T Director position throughout the program, especially during the first several months. This fact contributed substantially to the difficulties encountered in effecting liaison with outside agencies.

Unable to place clients in outside education and training agencies, and believing that the remedial reading and math which the participants needed could be supplied more effectively by their own staff, Vera, AYES, and CEP decided to make most E&T placements in-house. The Learning Center, a one-to-one tutorial service at CEP with six staff members and approximately twenty students, was converted into a large-scale classroom operation for Model II and III AYES participants. Initially, the Learning Center was directed by a professional educator; or the initial staff consisted of non-professionals with little or no classroom experience. In January it was reorganized and expanded to handle as many as 100 students at a time. In the interim, however, the staff at the Learning Center were soon inundated with AYES participants (as many as 130 clients at one time during the peak months of November and December). For a while, then, the massive number of AYES students could not be accommodated by the Learning Center. During that time period, according to interviewed staff, personnel at the Learning Center functioned more as "policemen" than educators, and the quality of the education provided was generally perceived as poor.

By February, however, new personnel, improved organization, and a reduction of clients enabled CEP and AYES staff to turn the Learning Center into a more effective classroom setting. Nevertheless, several problems common to remedial education programs remained. These included: a constantly changing clientele with widely disparate abilities and attitudes; too many clients for the remaining staff to handle adequately (even after the number of students was reduced during phase two); an insufficient amount of time to train staff as fully as desired; and clients with severe psychological problems constantly disrupting classes.

The general consensus among interviewed program staff was that the Learning Center was chaotic and ineffective in the beginning, but worked "as well as could be expected under the circumstances" after a few months of operation.

An estimated 80 to 90% of all Model II and III participants were placed at the Learning Center. In addition, many Model I participants were placed temporarily at the Learning Center until work experience placements could be located for them.

Most Model II participants (76%) were placed in educational slots during the program. Approximately 54% were placed in GED classes and 22% in remedial classes; there were no placements made into English-as-a-second-language (ESL) classes. During the first few months of program operations, the vast majority of these educational placements were made in-house at the Learning Center. During the latter part of the project, AYES staff were able to place more clients into alternative schools such as LUCHA and the 93rd Street School.

According to program staff, who were interviewed, most clients placed at the Learning Center during the first few months of program operations did not significantly improve their reading levels or make progress towards their diplomas; the Learning Center was said to have been somewhat more effective later on in the course of the project. Program staff evaluated the quality of the outside educational agencies as varying from quite good to poor. However, neither the quality of the agencies nor the progress of the clients were systematically evaluated or monitored by project staff.

Since the ETS instrument package did not include a post-test reading measurement, the AYES database includes nothing on this variable. Therefore, it is impossible to empirically evaluate participants' gain scores in reading level. Very few Model II's received an occupational certificate (6%), a high school diploma (2%), or a GED (6%) while in AYES. Indeed, this was not a program goal for all Model II clients, and program staff have said, however, that the short duration of program participation (six months maximum participation) and the generally low literacy levels of AYES clients combined to make it extremely difficult for clients to obtain their diplomas while in AYES. Random assignment to model, despite claims of program staff to the contrary, did not lead to a significant increase of negative terminations.

AYES staff were not as successful as hoped in locating vocational training slots for Model II participants. About 22% of all Model II's received vocational training. These slots included: auto mechanics, data processing, secretarial, nurses' aide, plumbing, and refrigeration. Although AYES staff generally deemed the range of vocational training slots as inadequate in number for their clients needs, they seemed satisfied with the quality of those placements.

As a final note on Model II, it appears that the basic experimental design was not always followed. Several clients, especially those with diplomas, were said to have had no acceptable E&T slot available for them. This problem was exacerbated by random assignment to model. (As stated previously, it was difficult to locate vocational training or community-based education classes for AYES clients.) According to AYES staff, some of these clients were terminated from AYES, and their counselors attempted to locate other programs for them.

Others remained as de jure Model II or III participants, but in actuality were placed in work-experience slots, usually at CEP. Evidence corroborating this comes from three sources: interviews with E&T staff; interviews with program participants; and data indicating at least thirteen clients with high school diplomas being placed titularly in either GED or Remedial slots at the Learning Center.

D. Model III

Model III was designed to offer program participants a combination of both work experience and education or vocational training. All Model III participants were expected to receive approximately equal exposure to both model components. Virtually all Model III participants in New York were placed in educational slots for their E&T component: 73% in GED and 22% in Remedial. The remaining 5% were placed in vocational training slots. Most Model III's (79%) worked at single-site placements for their work experience, with the remainder participating on work crews. In general, Model III's spent 2-3 months first in one model component and then 2-3 months in the other. According to program staff, Model III participants received equal exposure to each model component; nevertheless, given the policy having participants spend blocks of 2-3 months in each component, early program terminees could not have possibly received equal exposure to both E&T and work experience. Unfortunately, there are no data available indicating length of each individual's participation within each model component.

Although, according to the Grant Plan, Model III participation was supposed to be scheduled in either split-day or sequential two week placements in E&T and work experience, implementation of that plan proved impossible for a variety of reasons. First, program staff claimed difficulty locating work experience slots for Model III's during the first few months of program operations because work crews were already filled up with Model I's and they couldn't locate a sufficient number of single-site placements. As a result, many Model III's spent their first months of AYES participation in educational slots, usually at the Learning Center. Program staff were similarly unsuccessful finding vocational training slots for Model III participants. Transportation problems added another obstacle: it was virtually impossible to schedule split-day E&T and work experience placements accessible not only to each other, but also to the participant's home. Random assignment was claimed by interviewed staff to have further complicated the development of effective Model III placements. This last assertion is not corroborated by available program data: within Model III the percentage of negative terminations of guided choice (52.9%) participants was higher than the rate for random assignments (39.2%).

Finally, program operators were unable to develop Model III placements as flexible as those called for by the Grant Plan. That document indicated that a participant would not spend more than 2 weeks at a time in an E&T slot before switching to a work experience slot, or vice-versa. In fact, participants spent considerably more time in one type of slot before switching to the other type. In the opinion of the interviewed staff, these problems precluded the development of Model III slots which maximized the merits of a combined work experience and educational program model. According to program data, virtually no Model III's received a high school diploma, a GED, or an occupational certificate; moreover, Model III's averaged the fewest hours of program participation of the three New York models (although the positive termination rate was no lower than the other two models). It is possible that a longer and smoother planning period might have lessened some of the implementation difficulties; however, most AYES staff in New York believed that, as structured, Model III was not viable within the constraints placed by the basic program design.

E. Counseling and Terminations

According to the Grant Plan, each AYES participant was to receive four hours of vocational counseling per week. In addition, all clients were to receive Adkins and VITAS evaluation sessions. The former was an instrument designed to increase participants' employability; the latter was a vocational assessment system. The principle goal of counseling, Adkins, and VITAS was to "develop short and long term employment goals for each participant and a plan for achieving those goals (p.10)."

The number of vocational counselors originally proposed for the program was cut because of budget constraints. The resulting high caseloads and the variety of other responsibilities delegated to Vocational Counselors made adhering to the original plan impossible. Program records are not sufficiently reliable to assess accurately the frequency and duration of vocational counseling sessions, interviews with program staff and a review of a sample of participant folders indicate that most clients did not receive four hours of vocational counseling per week. Moreover, as discussed below, both Adkins and VITAS testing were significantly abridged. Due to their proximity to the counseling staff, most clients in E&T spent more time with their counselors than did clients in work experience. (Of course, WSS served as unofficial counselors for clients placed on workcrews.)

The counselors had a two-fold role to promote clients' employability, or "job readiness." First, they determined the best possible placement for a particular client within the range of available model slots; of course, random assignment and the difficulty of locating E&T slots severely restricted these efforts. Second, counselors tried to teach their clients proper work-related behavior (e.g., how to dress for a job or job interview, how to deal with conflict on the job, what employers expect from their workers, etc.). Based on a review of a sample of participant folders and interviews with program staff, it does not appear that AYES consistently established thorough, comprehensive Employment Development Plans (EDP's) for program participants.*

Several factors complicated the counseling efforts at AYES. Most basically, there was considerable controversy among those planning and operating the AYES project over the type of counseling required for this client population. According to Vera Program Officers, counseling in AYES should have been almost exclusively vocationally-oriented. Most members of the E&T staff felt that Vera's definition of counseling was "too narrow" and that vocational and personal counseling had to "go hand in hand." Some counselors reported feeling that Vera discouraged personal counseling, although the Vera Program Officers denied this. The counselors' attitudes reflect the basic philosophy of CEP prior to the implementation of AYES, namely that the vocational goals of many AYES clients could not be attained without concomitant personal counseling. Vera Program Officers countered this argument by asserting that this project did not have the resources to offer extensive personal counseling, nor did most of this personal counseling directly address AYES' vocational goals. Regardless, it appears that counselors spent a great deal of time conducting personal counseling with their clients.

Most counselors complained that they had too many roles; moreover, there was, especially in the beginning of the project, a lack of clear-cut systems and procedures for implementing those roles. For example, counselors had considerable intake duties, placement responsibilities, paperwork, and (for some) Adkins and VITAS assignments; moreover, at various points in the program's duration, the counselors had case loads of over 50 clients. One staff member expressed the belief that there were so many tasks that the counselors often did not know what their role was on the AYES project.

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* EDP's are a system for assessing at intake a participant's skill and interests, developing these attributes throughout that individual's program participation, and locating post-program employment related to these skills and interests.

Although disciplinary termination guidelines were specified in the AYES brochure given to participants, there was a wide latitude of possible interpretations of those guidelines (as discussed below in more detail). Several members of the program staff alleged that there was no clear-cut process for making terminations and no clear designation of responsibility for making these decisions.

According to the E&T staff members interviewed, there was confusion over scheduling Adkins and VITAS sessions and what was the correct policy for dealing with missed sessions. The "disorganized" and lengthy intake process, random assignment, the difficulty locating E&T placements, friction between the counseling and field staffs over terminations and discipline, and (especially in the beginning) difficulty monitoring outside placements were other complaints made by the counseling staff.

Attitudes towards VITAS and Adkins were mixed at best. The former is a "vocational assessment system that includes a battery of hands-on activities to be used in a simulated work environment" (Field Memo #3). It was judged by the staff members who were interviewed to be too lengthy, requiring too much space and personnel, and ineffective. Adkins is a "life skills employability series...designed to help disadvantaged adults and adolescents choose, find, get, and keep jobs" (Field Memo #3). Most members of the E&T staff (as well as several members of the field staff) felt Adkins was also ineffective and difficult to schedule and implement. Adkins was reportedly considered boring and irrelevant by most participants. According to a Deputy E&T Director, the whole process of scheduling and conducting Adkins and VITAS sessions was "disorganized." According to a program counselor in charge of VITAS testing, about 65% of all AYES clients received VITAS or Adkins. Due to time, space, and personnel limitations, both Adkins and VITAS were drastically abridged, thereby changing their content (according to the E&T Director). According to interviews and a review of participant files, it does not appear that VITAS or Adkins were used comprehensively to facilitate vocational counseling (except in cases where a client performed particularly well or poorly on one of these instruments). There were no systematic records kept on Adkins or VITAS counseling sessions or of their use by program counselors and job developers.

Termination policy was a complex and controversial issue. As stated in the brochure distributed to all AYES participants, a client could be terminated for any of the following transgressions: violence or threats of violence; possession of a weapon; stealing; drug or alcohol use; sexual misconduct; any unexcused absences; two unexcused latenesses; two negative evaluations from either work or school; not adhering to the dress code. The controversy resulted from different interpretations of that document as well as disagreement over the utility of its stipulations. Vera Program officers favored a stricter interpretation of the disciplinary system than did site personnel; moreover, AYES counselors and most long-time CEP employees considered this strict interpretation inappropriate for AYES clientele and contrary to the philosophy of CEP. These differences of opinion, in conjunction with the lack of clear-cut procedures for implementing termination policy, led to philosophical disputes both between Vera and site personnel and between AYES field and counseling staffs. Approximately 43% of the terminations at CEP were classified as negative. Program model and type of assignment to model had no effect on type of termination. Due to inconsistent coding procedures, however, it is impossible to approximate how many of these negative terminations were for disciplinary causes, as contrasted with the client's unannounced decision to leave the program.

Several other factors were cited as explanations for AYES' lack of adherence to Vera's termination guidelines: counselors frequently lacked adequate records of clients' attendance or performance; case loads were often too unwieldy to allow for close monitoring of clients; the counselors' ability to effectively monitor clients was impeded by their other functions (especially during intake); some counselors' sympathy for their clients made them reluctant to recommend any terminations; and the limited range of model slots and random assignment led many AYES staff members to favor a more tolerant outlook.

According to interviewed AYES personnel, most clients were consistently given the benefit of the doubt in regard to violations of the discipline code. Later on in the project, a somewhat stricter interpretation of the rules was instituted, although not quite as strict as Vera Program officers recommended. The New York Project Director stated that he would rather retain a client too long (after violations of this code) than terminate that client prematurely (when, presumably, that client could still benefit from AYES). Moreoever, the final E&T Coordinator, in reference to the confusion in AYES, averred that AYES clients "did not come into a structured environment...and it was not fair to hold to the rules that tightly when the operation wasn't running that tightly to begin with." He added that rules were "bent to their fullest" and served as "guidelines" rather than "the Bible, per se." According to some counselors and senior staff, rigid enforcement of these rules would have totally depleted the clientele of AYES. On the other hand, Vera Program Officers felt that a stricter interpretation of these rules would have set an example for the rest of the clients, who would have been more disciplined after seeing some of their peers terminated for infractions of the AYES guidelines.

By far the most common reason for negative terminations was excessive absenteeism. Other, but far less common, reasons for termination included: incarceration, drug problems, unwillingness to accept supervision, and poor performance. There were few cases of violence or threats of violence. Some clients terminated by their placement agency for poor performance were retained on the AYES project, but were transferred to other placements.

The procedures for making negative terminations depended on a number of factors: the type of problem, the participant's program placement, at what point in the program's existence the problem arose, and whether any extenuating circumstances surrounded that problem. In cases of absenteeism, the counseling staff, after being notified of the problem, tried to contact the client, ascertain the cause for these absences, and attempt to induce the client to return to AYES. It appears that the vast majority of clients with excessive absences were given the opportunity to improve their attendance record, since most counselors felt that the AYES attendance guidelines were too strict. In many cases involving absenteeism or some other transgressions, the client and his/her supervisor or counselor wrote a "contract" stating that the client would be terminated if he/she violated the terms of that contract.

During the first few months, the counseling staff made terminations themselves. Later other procedures were instituted. If the client was placed in the field work component of AYES, the WSS or Field Rep made a recommendation of termination to the Director or Deputy Director of Field Operations, who made the final decision over a particular termination. If the client was in the E&T component, the counselor sent a recommendation to the Director or Deputy E&T Director, who then made the actual termination. Although official terminations required the signature of the Project Director, he was actively involved in the decision to terminate a client only in special cases.

Since the counselors and WSS frequently had divergent views about termination policy, disagreements between the two groups arose. As stated by one AYES senior staff member, the relationship of the WSS to the clients was that of an "employer," which sometimes conflicted with the more "advocate" relationships of the counselors. As a result, clients sometimes played one against the other, a tactic abetted by the lack of consistent communication between the two staffs. While the counselors sometimes complained that the WSS were "insensitive" to the needs of their clients, the WSS sometimes accused the counselors of eroding their (the WSS') authority over the clients. It appears, however, that the decision to let Field Operations Directors handle their own terminations attenuated much of this conflict.

Table 4 presents data on positive termination rates in New York. Plausible explanations for the increased positive termination rates in Models II and III are the increased range of E&T slots and improvements made in the Learning Center during phase two of program operations.

TABLE 4

PERCENT POSITIVE TERMINATIONS

	Model			
	I	II	III	TOTAL
Guided Choice	49.3	56.3	54.6	53.5
Random	44.6	68.4	70.2	60.3

F. JOB DEVELOPMENT

The Grant Plan for CEP specified that all AYES clients would receive job placement services beginning in their third month of program participation. The goal of job development would be "to place all the participants in jobs or skill training programs that relate to their immediate employment goals (p.11)."

Eleventh hour budgetary negotiations with DOL and eventual cut-backs, combined with other programmatic limitations, resulted in job development services characterized by interviewed Vera and AYES staff as "inadequate" to "disastrous." Optimally, job developers should have been hired prior to program implementation in order to establish necessary job contacts, but budgetary complications precluded their hiring until January, when the program was several months underway. Moreover, these initial job developers proved ineffective and were soon terminated.

As an additional complication, the job development operation was contained within the E&T department, which was also plagued with personnel problems at the senior level (see E&T section). Without an acting E&T Director for several months of program operation, responsibility for job development rested with a staff member who had no prior experience in this area.

Until the job development unit underwent reorganization in March, AYES reported virtually no post-program placements into unsubsidized jobs. In addition to the personnel problems mentioned above, several other factors contributed to the general ineffectiveness of the job development operation:

1. There was no consensus among the program staff about who was responsible for promoting the job-readiness of terminating participants. According to some staff, the counselors were responsible for promoting clients' job-seeking and job-holding skills; and only those clients who were job-ready were to be sent to

job developers. Other staff members were under the impression that job developers and counselors were to work together to promote job-readiness of terminating clients. In addition, Adkins training sessions were not consistently administered during the first few months of program operations, and reportedly many clients never received Adkins Training (see Counseling). Both AYES managerial staff and Vera Program Officers believed that the level of vocational counseling in the program was inadequate.

2. There was an apparent lack of systems, records, and organization for the job development process in AYES. There were no regular channels of communication between the field operations and counseling staffs and those responsible for job development. As late as June 1981, the WSS, counselors, and field representatives showed little understanding of the job development process and reported that they had only infrequently exchanged information with job developers. AYES maintained no systematic records of job development or placement services, no stated guidelines for evaluating clients' job-readiness, and no system for scheduling pre-job placement interviews with terminating clients and their counselors. There were no post-program follow-ups on the few terminated clients informally placed in jobs by counselors or field staff. There were no stated criteria for matching clients with jobs. In fact, the person in charge of job development from January to March stated that she was unsure which criteria were used or should be used for matching clients with jobs. It appears that no one took charge of implementing a more efficient system for job development until the hiring of a new E&T Director in March.

3. The characteristics of the AYES participants, combined with the depressed state of the local job market, made placements difficult even under optimal program conditions. Most AYES clients were young minority group members, largely unskilled and semi-literate, and often with court records. Moreover, many

employers in New York were laying off employees, particularly those with semi-skilled or unskilled positions. Whether due to the participants' personal characteristics, the limitation of six months' maximum participation in the program, or the general inefficiency of AYES (at least during the first half of the program's existence), most terminating clients were deemed by interviewed AYES staff as not yet job-ready.

In March the new E&T Director, possessing some experience in the area, attempted to implement a new job development and placement system. Nevertheless, most line-staff interviewed in June were not particularly positive or optimistic about the job development operation. Still without any job developers during the months of March to May, AYES reported only 16 placements into unsubsidized jobs during this time.

The reorganization of the job development operation included several personnel and procedural modifications. Most important, two experienced job developers were hired in June, 1981. Second, a system of rating job placements was instituted. Each developer was expected to produce a total of at least two placement points per week; placement into an unsubsidized job counted for one point, placement into skill training for one-half point. Third, they attempted to institute systems for regular exchanges of information between line-staff (especially counselors) and job developers. Fourth, the new job developers made a concerted effort to contact potential employers, locate positions for AYES participants, and match those positions with terminating AYES clients. These efforts met with moderate success: a total of 57 clients were placed in jobs from June to September.

Certain impediments to successful job placement efforts were never completely overcome -- the most obvious and least controllable factor being the inherent difficulty of locating positions for hard-to-place clients in a depressed job market. The efforts of internal reorganization met with only modest success, perhaps due to the formidable task of implementing a new system while program operations were already well-underway.

In process analysis interviews conducted as late as September, it was the impression of job developers and senior staff that many, if not most, terminating participants in AYES were not yet job-ready. Many clients had minimal literacy skills, did not know what a resume was, and did not know how to complete a job application. Nevertheless, AYES offered job placement services to all terminating clients (except those terminated for disciplinary reasons) who were cooperative and reasonable in their expectations.

Not only were many clients hard to place in jobs or skill-training programs, but also clients often missed scheduled interviews with their job developer or prospective employer. This situation obviously compromised the job developer's contacts with employers and wasted valuable time and effort. Both job developers claimed that Adkins offered training that was at too high a level for the generally low-level positions that AYES clients were seeking; in addition, they noted that as many as 25% of all clients referred to them had not undergone Adkins training. (Interviews with program staff and a review of a sample of participant folders lend credence to this last assertion.)

There were differences of opinion about the relative job-readiness of participants in the three models. The Project and E&T Directors asserted that Model I clients were most job-ready since they had work experience. One job developer saw no inter-model differences in job readiness, but also admitted not knowing the program model of many of her clients. The other job developer perceived no differences among the three models, but noted that clients placed in the Learning Center were the least job-ready participants.

The jobs located for terminating clients tended to be semi-skilled blue-collar positions, usually at the entry level. Most employers were small construction firms, wholesale and retail firms, and factories. Some clients were placed in clerical positions. One job developer noted that by the time AYES was ready to make placements many firms weren't hiring because they were doing summer inventory.

Although the communication between the E&T and field staffs and the job developers was significantly improved after the reorganization of the job development process in June, the role controversy between counselors and job developers in promoting job-readiness of AYES clients was never fully resolved. Perhaps resulting from this disagreement, the job developers complained that the counselors sometimes did not adequately screen out clients who were not job-ready. In addition, some counselors were accused of not presenting to job developers an accurate assessment of their clients' job-readiness or general psychological well-being.

The match of job openings and terminating clients was done in several ways. While the client usually accepted the judgment of the job developer, the counselors and job developers often had to convince clients to have more realistic expectations of the salary and job they could obtain. There was no extensive follow-up system for job placements; follow-ups were made sporadically, and they resulted from the personal relationship between client and counselor or job developer rather than any systematic procedure. Several clients had been placed in jobs more than one time. Job development services were offered to AYES clients, including those already terminated, throughout the month of September, October and November.

CHAPTER THREE: MIAMI

A. INTAKE

Intake in Miami began on July 21, 1980 with the expectation (according to the Grant Plan) that the initial intake period would last eight weeks. As in the other AYES sites, intake was much slower than had been expected. The initial intake period, which used guided choice model assignment, lasted for 15 weeks, ending on October 31, 1980. At this point 225 experimentals had been taken into the Miami project. The second phase, during which experimentals were randomly assigned to model, began on November 3, 1980 and ended on March 30, 1981. During the second phase of intake, the Miami site assigned 181 experimentals. After adjustments for no-shows, there were a total of 377 participants in the Miami AYES program.

Forty-nine percent of the Miami subjects were criminal justice (CJ) referrals, almost meeting the Grant Plan requirement of 50%. Although site staff had attempted to attract a greater percentage of CJ referrals, they were not successful. Characteristics of the program participants are presented in Table 5.

Outreach

Outreach was accomplished by making presentations at community and criminal justice agencies to let them know about AYES, using the media (especially public service announcements), and relying on "ad hoc" recruiting teams (composed of AYES staff members) who went to places where they were likely to find candidates for AYES (e.g., street corners, candy stores).

Most candidates were referred by criminal justice agencies (e.g., Pretrial Intervention, Juvenile Restitution Program, jails, court-related agencies, Health and Rehabilitative Services (HRS), and Manpower Centers (CETA). There were also a substantial number of walk-ins as a result of the street recruitment

TABLE 5 (N=372)

Characteristics of Miami Participants

Referral Source	
CJ*	46.3%
Other	53.7%
Assignment Type	
Guided choice	57.7%
Random	41.3%
Sex	
Male	66.6%
Female	33.4%
Had High School Diploma	
Yes	17.0%
No	83.0%
Ethnicity	
White	2.9%
Black	74.9%
Hispanic	22.2%
Mean Age at Intake	18.7

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* While the percentage of CJ referrals is 46% for the experimental group, for the Miami sample as a whole, it is 49%.

effort. Unlike New York, where intake was very slow at first but accelerated rapidly during phase two, or Albuquerque, where intake continued to be slow throughout the project, Miami's intake remained steady throughout. In part, this reflected the fact that the program had support for a full-time Intake Coordinator and that the State Attorney's Office actually assisted the program in a variety of ways. The total number of participants in Miami (377) did fall short of the projected total of 450 participants. More than one staff member indicated that there was a lack of resources for recruitment once the program began. (For example, the worksite supervisors who had formed the "ad hoc" recruitment teams now had crews to supervise.)

While Miami AYES staff did not mention differences between phase one (guided choice) and phase two (random assignment) participants, the former Project Director did indicate that, during the latter phase, the percentage of non-criminal justice referrals increased. This perception is supported by the data in Table 6. In addition, there were two other significant differences between phase one and phase two participants: (1) a higher percentage of guided choice participants (75%) than randomly assigned participants (57%) had worked at some time prior to intake; and (2) guided choice participants had significantly higher mean STEP reading scores than randomly assigned participants. This may be in part due to the higher percentage of recent Haitian immigrants during phase two. In any case, it suggests that there could be some outcome differences between participants who entered the program during phases I and II.

Eligibility

To be eligible for participation in the AYES program, an individual had to be between the ages of 16 and 21, out-of-school, out-of-work, and CETA-eligible. In the Miami site, eligibility screeners were provided by the local prime sponsor, the South Florida Employment and Training Consortium (SFETC). Thus, the screening was done by people from outside the program and the individuals doing the screening changed from day-to-day. As a result, the quality of the eligibility screening may have varied over the course of intake; in general, according to the Project Director, the eligibility staff were very helpful, and the standards used for eligibility screening were consistent over time. Sometimes there were problems getting enough eligibility workers, which could add three hours to applicants' waiting time.

The Miami program had a substantial number of Cuban (10.7%) and Haitian (8.0%) immigrants who applied for the program. Since most of these were recent immigrants (the mean age of arrival of the Miami foreign born subjects was 15.0), it was necessary to conduct the eligibility screening (and the rest of intake) in Spanish and Creole. While a few of the program staff members could translate into Spanish, none of the program staff spoke Creole or French. As a result the Vera Research Associate was often called upon to translate for the Haitian applicants; this additional responsibility caused some problems for the research staff, who had other intake tasks to complete.

Intake Process

The intake process lasted about 15 hours, spread over a two-week period. The first day of intake consisted of an introduction to AYES, eligibility screening, and administration of research instruments. The introduction to the program was conducted by the Project Director (or another staff member) and the Research Associate. The program was described briefly, and the applicants were told that they had a 50% chance of getting into the program. The random assignment process was stressed (during phase two), and any questions were answered. Applicants then proceeded through intake, with AYES staff checking their documents before sending them to the eligibility workers. As indicated above, the shortage of eligibility workers and many non-English speaking applicants slowed the process.

TABLE 6

Comparison of Phase One and Phase Two

	Phase One (Guided Choice) N=225	Phase Two (Random Assignment) N=152
Referral Source		
CJ	48.6%	42.1%
Other	51.4%	57.9%
Sex		
Male	68.2%	64.0%
Female	31.8%	36.0%
Ever Worked*		
Yes	75.3%	56.9%
No	24.7%	43.1%
Most Recent Job Labor Market Segment		
Primary	35.4%	30.9%
Secondary	64.6%	69.1%
Mean Age	18.8	18.6
Mean Step Score**	9.8	7.2
Mean Highest Grade Completed	10.3	10.3

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* $X^2=14.189$; df=1; p=.0002

** F(1,374)=15.09; p=.0001

As applicants were certified eligible, they were sent to research for the individual Vera interview and ETS testing. Research intake interviews lasted between 10 and 45 minutes, depending on the extensiveness of the person's work history and his/her cooperation. The ETS tests were administered in groups (sometimes there were English, Spanish, and Creole groups run simultaneously) and lasted about an hour.

On each "Day 1" after all applicants had completed the research instruments, the site Research Associate, using random number tables, assigned applicants randomly to the experimental and control group. Those applicants assigned to the experimental group were informed that they had been accepted into the program and should see a program staff member about an appointment to continue the intake process. Controls were paid a $10 stipend and received a debriefing.

In the beginning of intake, debriefing of controls was done in groups. Soon, however, it was changed to individual debriefing because it was "less problematic and easier to control the kids." Debriefing served four purposes: emotional support, referral, and payment of the stipend, and establishing the basis for maintaining future contact. AYES had a list of the various CETA programs and their requirements; this list was used to refer controls to other programs. Furthermore, the person was told that if a referral didn't work out, he/she should return to AYES for another referral. As time went on, however, and CETA was decimated by cutbacks, the list of referral sources dwindled, and the primary referral became the Youth Opportunity Center. In addition, debriefing was always a problem for the Haitians (and for non-English speaking Cuban refugees) because there were no programs to which they could be referred. The site research associate indicated that debriefing took a good deal of his time because he was called upon to translate for the Haitians. Another problem identified by the Research Associate was that sometimes there were no program staff members available to do the debriefing; thus, research staff had to stay with the applicants until someone could be found to perform this task.

During phase one of intake, experimentals had a guided choice interview with a program counselor to determine their preferred model choice(s). (This occurred on Day Two of intake along with the TABE Reading and Math Ability Test.) As in the other sites, model preferences were then sent to the site researcher, who assigned experimentals to model each Friday. An attempt was made to offer the candidate his/her first choice (no one received worse than a second preference), while also assuring that the three models filled up evenly. (While in the planning phase of the program there had been some concern that one model might be widely preferred over the others, this did not turn out to be a problem. The great majority of participants received their first choice.) For research purposes, this practice assured all guided choice experimentals an equal chance of receiving their model preferences; programmatically, this system enabled the program operators to avoid the potential staffing problems generated by widely divergent model sizes. In addition, each Friday the site researcher monitored the proportion of criminal justice referrals in the experimental and control groups.

One problem mentioned by both program and research staff was a lack of space during intake. Since intake lasted for a total of eight months, this was of some consequence. The research staff had one office for interviews and the use of a conference room for ETS testing. When intake was heavy, however, there could be up to four research interviews conducted simultaneously. Since sensitive material was covered in these interviews, it was necessary that they be conducted in private. The counselors were asked to make their offices available to research staff upon request. Once the program started, this caused some hardship for the counselors. Since space was a problem for the program as well as for research, the problem was never fully resolved.

Intake to Program Start

It took two weeks after assignment to the experimental group before a participant could begin program participation. According to both the original Project Director and his replacement, this was intentional. This time was needed to conduct TABE testing, conduct the guided choice interview, arrange for participant physical examinations, and to complete paperwork required by SFETC, Vera, and the AYES program. During this two-week period participants went through an AYES orientation in which the program rules and procedures were explained. The participants were not paid for this time, but program staff did not think it was a problem.

During phase one of intake, each client went through a guided choice interview with his/her counselor. In this interview the client and the counselor established which model, and what type of activity within the model, was most suitable for the client. In addition, clients met with counselors to establish the goals of that client's AYES participation. During phase two intake, the "guided choice" interview attempted to work out for the participant the optimal program placement within the restrictions of his/her random model assignment.

B. FIELDWORK AND COMMUNITY RELATIONS

As in the other two sites, Miami AYES offered two types of Model I placements: AYES-supervised workcrews, where participants were taught basic construction skills; and "individual" (or "single-site") placements at non-profit or government agencies where participants were given work experience in a variety of traditional CETA-type jobs. Placement in one of these two types of slots depended on the participants' desires and needs (as assessed by the Field Representative). All participants placed on these crews were male. About 70% of all Model I participants were placed on workcrews.

The Miami AYES program differed from the other two sites in that worksite supervisors functioned as official counselors for Model I participants. Thus, the only contact Model I participants had with the counseling staff was for Adkins and VITAS sessions; the worksite supervisors conducted "rap" sessions for participants on their crews and tried to help them with personal problems.

The principal goal of Model I was to teach participants proper work habits so that, upon graduation from AYES, they could get and hold a job. This objective was operationalized in different ways by the interviewed AYES staff, but included providing a situation as close as possible to a "real" work situation; counseling participants in job holding skills; offering participants work experience; improving participants' work performance; and teaching participants to enjoy work.

Placement on workcrews had advantages and disadvantages relative to individual placements. Workcrews were supervised by AYES staff, thereby providing for more direct control over the participants. Another advantage of being on a workcrew was learning basic construction skills. On the negative side, the second Project Director indicated that this close, intense supervision raised the potential problem of "cradling" participants, treating them as if they were in a program rather than on a job. The original Project Director supported this, stating that workcrew placements had more disadvantages than advantages, that participants who worked in crews with their peers "got away with a lot more." While this observation cannot be tested directly, a comparison of the negative termination rates for these two groups does not support it. Program data indicate that 57% of the Model I work crew participants were negatively terminated as compared to 37% of those on single site placements. (With $X^2=3.66$ and df = 1, this approaches the .05 significance level.) If workcrew members were "getting away with more," the termination data don't show it.

While teaching participants how to behave on a job was the principal goal of Model I, workcrew participants were taught a variety of construction skills. These included painting; plastering; sheetrocking; erecting framework for partitions; using manual and power tools; carpentry; masonry; pouring cement; roofing; interior demolition; paving driveways; and safety precautions.

The major workcrew project in Miami was complete renovation of the Coconut Grove Automotive Training Center. Many workcrews were assigned to the site and it took over eight months to complete. The work covered all phases of construction including roofing, sheetrocking, installing door frames, painting, and grounds cleaning. Other major workcrew sites were the Model Cities Methadone Clinic (painting and landscaping), the Elizabeth Curtis Day Care Center (exterior and interior painting), and the Overtown Day Care Center (painting).

Instructions at the worksites were given by the worksite supervisors. The methods of instruction varied from supervisor to supervisor. For example, one site supervisor indicated that he told participants what to do and then checked their work. Another said that he would often have a participant with experience in the task demonstrate it for the other participants.

The participants' ability to learn the skills varied; however, one site supervisor felt that "those who want to learn do." Assessments of the general quality of work varied considerably. One site supervisor thought the quality of work was "good," another "fair" (due to the poor quality of materials and the lack of proper equipment). The Project Director labelled the quality of work "mediocre." He felt this was due to lack of training of both participants and supervisors; inadequate planning; and difficulties in terminating participants due to the prime sponsor's requirement of an 80% positive termination rate for all programs under its auspices (see later section on terminations). The Field Operations Director believed that participants' work habits improved over time. As they learned more about how to do the task and could see changes in the quality of their work, their attitude improved. He also indicated that when there was work to do, morale was high. Morale was low when they were not fully occupied. One of the worksite supervisors had another perception: when the participants first started, they were highly motivated. "Once they thought they knew everything, their attendance declined. They would go out on their own looking for a job; when they were disappointed in their search they would return to the worksite." Participants were described as having good, close relationships with one another; fights were rare. The relationships between the participants and their supervisors were reportedly good.

The major problem identified by the field staff was getting enough supplies; they also criticized the quality of materials provided by those organizations for whom AYES work was done and the absence of instructional materials. As a result, the participants learned how to do the work, but lacked the appropriate terminology. The worksite supervisors further indicated that they thought this deficiency would hinder the participants in finding construction work so they instituted a course to alleviate the problem. Other problems included the lack of a Spanish-speaking worksite supervisor and participants' absenteeism.

All of the interviewed field staff indicated that random assignment to model was detrimental to the quality of the program. They felt that random assignment placed people in work experience who didn't want to be there and these individuals often became discipline problems. The non-English speaking participants preferred ESL classes (Model II), which was frustrating for both participants and staff. Although the Field Operations Director believed random assignment increased the drop-out rate, according to the data, negative termination rate was not significantly related to assignment type. (See later section on terminations for further discussion.)

Those Model I participants not on workcrews were placed in "single site" placements in non-profit or government agencies. Site sponsors included Social Security Administration, Easter Seals, Dade Juvenile Detention Center, Liberty City Health Services, and about 15 other agencies. Participants placed in health care facilities gained experience in taking blood for tests, CPR, urinalysis, giving eye examinations, taking blood pressure, etc. Some participants obtained clerical skills such as typing, filing, switchboard operation, and operating office machines. Others worked as drivers or home health aides for the elderly. AYES participants in single site placements were supervised by agency personnel; however, AYES Field Representatives were responsible for making the placements, preparing and collecting timesheets, preparing disciplinary actions, providing supervisors with monthly evaluations, and delivering paychecks.

The major advantage of individual site placements as perceived by program staff was the exposure to the "real" world of work, allowing participants to work with people other than those enrolled in the program. Participants on individual sites were supervised less closely than those on workcrews; some staff members saw this as positive, others negative. While some single site placements were poorly supervised or offered meaningless work, most sponsors were seen as providing good work opportunities for the participants. Both field representatives felt constrained by having to place participants in non-profit or public sector positions; as a result, they could not always find a placement which fulfilled the participants' needs.

The quality of work at single site placements was described as ranging from "extremely poor" to "above satisfactory," depending upon the participants' skills. Like the worksite supervisors, the field representatives disagreed on how the participants' work habits had changed. One field representative felt that some participants' work habits improved over time. The other field representative noticed that about a month before program graduation, participants' behavior deteriorated and "they begin to get restless and start doing things they never did before" (e.g., not calling when they are going to be late). One field rep felt that morale improved over time; the other indicated that morale was generally high, especially among the Haitians. Random assignment was considered to have a negative impact on the program; both field representatives felt that all non-English speaking participants belonged in Model II, where they could be placed in ESL classes.

Relationships between AYES and the community were described as good, although some staff indicated that the program should have had more public exposure. There were also indications that some community members were apprehensive about having an "offender" population work in their neighborhood; however, once they saw the work the participants were doing, they became more accepting of the program. In addition many AYES field staff members were known to the community from prior work with other agencies. Since many of the participants lived in Liberty City (where the AYES office was located) and they behaved well on the worksites, the community had a positive attitude towards the program. The former Project Director indicated that the participants developed a good reputation by doing work in the community.

The field staff felt that the community was very supportive and tried to help the participants. For example, the South Florida Builders' Association developed an OJT program to teach AYES participants construction skills. After seeing the workcrews in action, some community members asked worksite supervisors to hire their own children. In addition, community residents wanted to hire participants to work in their homes on weekends. Local businesses hired some participants and helped others locate jobs. A field representative also reported that some sponsors demonstrated their concern for AYES participants by throwing going away parties for graduating AYES participants and helping them with their studies.

C. EDUCATION AND TRAINING

In the course of planning the project, Vera, AYES managerial staff, and the sponsoring agency (SFETC) expected to place most Model II and III clients in community-based schools and vocational training programs. As in other sites, selection of these placement agencies was to be based on their compatibility with the goals of the AYES project and their experience with high-risk youth.

In the Miami site, education and training placements were implemented as described in the Grant Plan. Placements were made at Miami-Dade Community College and in three institutions within the Dade County School system: Lindsey-Hopkins, Miami Skills Center, and Dorsey Skills Center. These placements were facilitated by the prime sponsor's assistance. Classes were offered in English as a Second Language (ESL), preparation for the General Equivalency Diploma (GED), Adult Basic Education (ABE), and a number of vocational training programs. The vocational skills included clerical, air conditioner repair, auto mechanics, electronics, nurse's aide, and welding. All of the interviewed staff agreed that the range of placements was adequate; the only lack was an intermediary level pre-GED program for those participants who were too advanced for ABE, but not ready for GED.

Miami-Dade provided the ABE, GED, and ESL classes, but several AYES staff members found that, with the exception of ESL, the Miami-Dade services were unsatisfactory. They provided classrooms and flexible hours, but did not purchase the required materials, provided poor teaching, and did not enforce discipline standards adequately. The former Project Director indicated that the quality of placements suffered because Miami-Dade was not prepared for the specific needs of the AYES population, nor were they able to accomodate the large numbers of AYES participants. As a result, as placement slots opened within the Dade County

School system, AYES participants were taken out of the Miami-Dade classes, and eventually the contract was cancelled.

The Dade County Schools were considered excellent. They provided good materials and excellent instruction, both of which helped improve the participants' attitudes toward school. In addition, they were described as having a "no-nonsense" attitude and enforcing the AYES discipline code. The former Education and Training Director evaluated the programs offered at each of the schools. He felt that overall the best program was the Miami Skills Center; this school provided excellent classes in ABE, GED, welding, and clerical skills. Lindsey-Hopkins was a trade center which offered good classes, and he considered the Dorsey Skills Center adequate in teaching but poor in discipline.

The only change in the availability of placements over time was the result of the cancellation of the Miami-Dade contract. The interviewed staff did not indicate this was a problem. Placements were made based on the guided choice interview, requirements of the institutions, and participants' TABE test scores. Participants requiring remedial help to get into GED classes were placed in ABE classes. Those desiring a high school diploma (and had the requisite reading and math scores) were placed in GED classes. While only 2% of the Model II participants obtained their GED, the AYES staff felt that the participants in GED classes made progress and evinced general improvement in reading and math skills. Non-English speaking participants were placed in ESL classes, which interviewed staff evaluated as very effective. A number of program staff members felt that the Haitians made the best use of classroom time; they had the highest attendance and motivation and caused the fewest problems. Those participants who had a GED or High School diploma were placed in vocational training programs. Participants in Model II received the following placements: 38% in GED classes, 25% in ESL, 19% in vocational courses, 8% in ABE, and 6% were not placed.

According to program staff, random assignment had a negative impact on the program due to inappropriate placements; however, the Project Director felt that its effect on Model II was less than that on Model I because most participants wanted classroom training. As noted in the section on terminations, random assignment did not apparently lead to an increase in the negative termination rate among Model II participants.

D. MODEL III

Model III participants in Miami were placed in a variety of educational and vocational slots: 52% in vocational programs, 20% in ESL classes, 17% in GED classes, and 11% in ABE classes. For the work experience component of Model III, participants were equally split between work crew placements (51%) and single site placements (49%).

The Grant Plan stipulated that Model III participants were to have equal exposure to work and classroom experiences; this could be accomplished by either split-day or alternating two week placements. In Miami most participants had split-day placements, three hours each of work and school per day. There were approximately 10 participants who never received the work experience component; according to program staff, these were generally non-English speaking Haitians or Hispanics who need ESL training. In addition, the former Project Director indicated that although participants received equal exposure to work and school, the quality of placements was sacrificed. The program could only place participants in a class that would accept half-day students, and the higher quality programs would not always accept them. The biggest problem for Model III participants was transportation; to alleviate this, Field Representatives tried to place participants in work and school slots close to each other.

Program staff claimed that, as in the other two models, random assignment had a negative impact on Model III. The negative termination rate does not support this assertion; while 45% of the participants who were assigned to Model III under guided choice were negatively terminated, the negative termination rate for randomly assigned Model III participants was only 30%. (Again, this is probably due to the imposition of the 80% positive termination requirement, which is discussed in the section on terminations.) In addition, while the overall positive termination rate was equal for Models II and III (61%), on the average Model II participants spent more hours (467) in the program than did Model III participants (388 hours). Since participants in both models were supposed to spend 30 hours per week in their placements, the reasons for this difference are unclear. Additional program data indicate that only 1% of all Model III participants received a high school diploma while in the program, 3% their GED, and 2% occupational certificates.

D. COUNSELING AND TERMINATIONS

The requirement of four hours of counseling per week was further articulated in the program manual developed by the Project Director. These four hours consisted of individual and group counseling, with the latter including Adkins and VITAS. Unfortunately, since the participant folders were not reasonably accessible to the research staff at the time this analysis was conducted, it was impossible to assess whether these requirements were met; however, interviews with AYES staff provided no reason to believe otherwise.

The AYES staff felt the goals of counseling were to maintain the participants' motivation; make the participants responsible individuals; prepare them to accept AYES training and post-program employment; and give the participants support, guidance, and an awareness of career alternatives. The Miami site differed from the other two AYES sites in that counseling responsibilities were formally shared by both Field Work and E&T staffs. Participants in Model II were counseled by the vocational counselors; Model I single site participants received counseling from the field representatives; Model I workcrew participants were counseled by worksite supervisors; and Model III participants were split among the counselors, field representatives, and work site supervisors. Participants in all three models received Adkins and VITAS training from the vocational counselors.

Model I participants received their personal and vocational counseling (other than Adkins and VITAS) at the work sites. Since program operators thought it was best to keep traffic in the AYES office at a minimum, counselors visited Model II participants at their school placements. In addition, the counselors called participants at night "to schedule appointments and to let them know that someone cared." The counselors also indicated that participants sought their help with problems related to such matters as childcare, money, abuse from a boyfriend or husband, or birth control. The counselors had a list of daycare centers in the Miami area and took participants to visit them. For other problems, the counselors referred their clients to outside agencies (e.g., to a therapist for psychological problems).

While there was no discussion of Employability Development Plans (EDP) in the process analysis interviews, the EDP appears to have been a specific part of the program design. The Program Manual indicated that the EDP was to be designed on Day Two of intake to determine the Model II participants' educational or vocational placement. Although the research staff did not examine the participants' files for EDPs, Vera program staff indicate that they were prepared and placed in the files rountinely.

The most common complaint by the counseling staff was that paperwork limited their time for client contacts. In addition, they felt that their caseloads were too large, and, due to space problems, they lacked privacy for counseling sessions. Management staff (Project Director and deputies) cited additional problems with counseling. The former E&T Director felt that the biggest problem was poor communication between counselors and worksite supervisors and job developers. The Project Director indicated that the pressure for positive terminations (see below) further hindered the counselors ability to do their job. Large caseloads and additional responsibilities (e.g., Adkins and VITAS) limited counselors' time for individual counseling activities.

Both counselors and management staff evaluated the Adkins system positively. The former E&T Director felt that it helped participants to make choices and set goals for the future; built self-confidence; and taught participants how to get a job (especially how to write a resume, behave on an interview, and communicate effectively.) Other staff members valued Adkin's use of videotape, which allowed the participants to role-play, review their performance, and receive feedback from counselors and other participants. The staff's criticisms tended to focus less on the content of the Adkins modules than on its implementation in AYES. The former E&T Director indicated that, because of other program needs, Adkins was not used properly until February 1981, six months after program operation began. He felt that more staff should have been involved, and they needed better supervision. Moreover, it was difficult to schedule for Adkins training those participants who were at work or school all day. One counselor felt that some of the materials and methods were inappropriate for those participants who couldn't write.

In contrast to the positive evaluations of the Adkins system, there was no general agreement on the utility of VITAS. Some staff members felt that VITAS was ineffective, too long, and not utilized correctly. In contrast, other counseling staff members felt VITAS was a valuable counseling tool, showing participants where their aptitudes lay and helping them learn about themselves. In addition, according to the E&T Director, the job developers used the results of the VITAS assessments in making post-program job placements.

Termination policy was a complex issue. As stated in the "Participant Contract and House Rules," a client could be terminated for any of the following transgressions: violence or threats of violence, carrying a weapon, stealing, drug or alcohol use, soliciting for drugs or gambling, being involved in sexual activities on the job, any unexcused absence, two unexcused latenesses, two negative evaluations from either work or school, failing to have proper equipment, or not adhering to the dress code. The interviewed staff all asserted that the original termination guidelines were followed. It is important to note, however, that the Grant Plan indicated that a participant could be terminated for infractions of the rules, thus leaving room for selective application. The Miami AYES program chose to modify the policy by developing the "mandatory minimum recommendations" for first infractions and recommended policies for repeated violations of the same rule. For example, a participant caught carrying a weapon would receive a penalty of a 10-day suspension without pay and was required to submit a 10-page essay; he would be terminated after the second such offense. The former E&T Director (as well as other staff) indicated that, while the guidelines never changed, their application did. That is, when AYES began the rules were strictly enforced; later, when the pressure for positive terminations increased, the discipline became more lax.

There was no consensus among the staff on the most common reason for negative terminations, and the data provided by the program do not permit for making distinctions among types of negative terminations. The interviewed site supervisors were most likely to recommend termination for excessive absence and insubordination. Other staff cited such reasons as fighting, drugs, disrespect for rules and regulations, profanity, lack of interest and motivation, and stealing. It is likely that various staff members had different tolerance levels for infractions of the rules, that some could be pushed further than others before requesting a disciplinary action.

The general termination procedure can be described as follows. When the participant's immediate supervisor within AYES (counselor, site supervisor, or field representative) became aware of a problem, he/she wrote a "disciplinary action" which was submitted to the Project Director for approval. The E&T Director (or Field Operations Director) reviewed the counselor's report for completeness, but the project director made the final decision to terminate a participant.

Throughout the process interviews with Miami staff, complaints were registered regarding the institution of the 80% positive termination requirement. When AYES began, although it was sponsored by the local Prime Sponsor (SFETC), the

AYES management staff agreed with Vera Program Officers on the need for a stricter discipline system than most CETA programs; therefore, rules and regulations were strictly enforced and participants were terminated for violations. As the year went on, however, AYES management staff began to plan strategies for obtaining funding from the local sponsor for a second year. At this point SFETC imposed the standard CETA requirement that 80% of the program participants be positively terminated, and indicated that the rate of positive terminations would be used as a measure of program success. The AYES staff, from Project Director to counselor, felt unanimously that this requirement undermined the effectiveness of the program. Since discipline infractions could no longer be punished by terminations, staff members felt that their hands were tied.

The 80% requirement also resulted in a new category of participants, those who were in "hold pending" status. These individuals had completed their program participation and were no longer on the AYES payroll. However, the program would not officially terminate these participants until they had been placed in jobs or a 60-day "hold pending" period had expired. The definition of positive termination was placement in unsubsidized employment or a full-time school program.

There is evidence from research data that the termination rate changed over time: 53% of the guided choice experimentals (who came into the program prior to October 31, 1980) received positive terminations as compared to 60% of the randomly assigned experimentals (who entered the program between November 3, 1980 and March 30, 1981). Therefore, the apparent relationship between assignment type and positive termination rate may be misleading. The increase in positive terminations might have resulted from the program's stabilizing operations after the start-up period. As can be seen in Table 7 below, this effect is present only for Model II and III participants; it is not clear why Model I is different.

TABLE 7

PERCENT POSITIVE TERMINATIONS

	MODEL			
	I	II	III	TOTAL
Guided Choice	49%	56%	55%	53.5%
Random Assignment	45%	68%	70%	60.3%

E. JOB DEVELOPMENT

The Miami AYES program had a well-structured job development unit and received extensive technical assistance in this area from Vera. For most of the duration of the program, there were two job developers; in addition, when the counselors' caseloads diminished in the late spring of 1981, some counselors were assigned job development responsibilities. The job developers worked closely with the field representatives to establish contacts with potential employees; often the field representatives referred job developers to contacts made during their search for single site placements. Unfortunately, however, many agencies were unwilling to hire non-subsidized workers when they could get free CETA help.

Job development services were available to all AYES participants, including some who were negatively terminated. (See discussion of "hold-pending" in Counseling and Terminations section.) Participants completing the full 26 weeks of the AYES program spent their last two weeks working extensively with the job developers. For this period they were expected to report to the job developer rather than to a worksite or classroom. This time was spent getting participants "job-ready" and then sending them out on job interviews. To prepare participants for interviews, the job developers used counseling techniques, some of the Adkins units, workshops, and role-playing. Participants were instructed on how to dress for an interview, what to discuss with a prospective employer, and how to fill out an application. They also taught the participants relaxation exercises to reduce anxiety during interviews.

The job development process involved close cooperation and communication between job developers and counselors, worksite supervisors, and field representatives. When a participant was ready to go into job development, his/her AYES supervisor referred him/her to the job developer. The job developer then asked the supervisor (who could be either a counselor, WSS, or Field Representative) for a recommendation and/or read the counselor's file on that participant. The supervisor provided an assessment of the participant -- the type of job for which the participant was suited; his/her skills and weaknesses; and problems the participant had (e.g., drugs, alcohol, or crime). This information was exchanged informally and used in conjunction with test results (Gordon Occupational Checklist) and participant's interests and desires to determine the type of job that would be most appropriate.

The job developer always made the initial contact with the employer, although a participant sometimes arranged his/her own interview. The job developer often transported participants to interviews in an AYES vehicle. If there were two or three participants qualified for the same job, all could apply but never at the same time. With the exception of department stores, it was program policy not to place more than two participants with an employer (because they were afraid the participants might get into trouble). After the interview, the job developer called the employer for an evaluation of the participant.

The type of job sought depended upon the skills of the participants. Since most participants were unskilled, the job developers found such jobs as service technician, shop helper, general warehouse helper, cook, nurses' aide, construction helper, stock clerk, and cashier. Those few AYES participants with clerical skills were placed in clerical positions. In general the job developer first reviewed the participant's skills, aptitudes, and desires, and then looked for an appropriate job. The former Miami Project Director indicated that job development is most effective by seeking existing slots rather than creating slots, that it is better to match participants with available jobs than to talk employers into creating new ones.

While all interviewed staff members indicated that there were official performance criteria for job developers, the reported criteria were not consistent across interviewees. One job developer thought she was required to make at least 10 employer contacts per week, two-thirds of which had to be in person. The other job developer said she had to make six personal visits and 12 (telephone) contacts per week. Both job developers stated that they were required to make two placements per week; in contrast, the former E&T Director said the requirement was four or five placements per month. The job developers did not think these requirements affected the type of jobs they sought, and felt they looked for jobs that were most congruent with the participant's qualifications. The E&T Director disagreed, stating that the performance goals affected the type of jobs sought. Regardless of the reason, all interviewed staff agreed that most jobs were of limited skill, entry-level positions. The only constraint (a CETA regulation, according to the E&T Director) was that they could not develop jobs that participants could get themselves (e.g., in fast food establishments).

Once a placement had been made, the job developer followed up on the participant. She made sure, first, that the individual had reported to the job, and then found out how he/she was doing. The job developer spoke to both the participant and employer, usually during the individual's first week of work. When talking to the employer about a participant's progress, the job developer also inquired whether there were other positions available. According to the Project Director, there was also a 30-day follow-up on participant progress; however, making placements took priority over these follow-ups. If a participant left a job, the job developer tried to find him/her a position with another employer.

While the job development process in Miami seemed to be implemented according to plan and was well-organized, the staff indicated a number of associated problems. Some of these were general, affecting all participants, and some were specific to various segments of the sample. The Project Director estimated that only about 50% of the participants referred to job development were job-ready. There was disagreement among the staff whether there were differences among the models in job-readiness. The Project Director thought that participants who had chosen Model II were more serious, gained more from the program, and were more job-ready than other participants. The E&T Director thought that Model II and III participants were more responsive than Model I participants to job development simply because they were in the office more frequently. In contrast, one of the job developers felt that job-readiness did not vary by model. While all of these opinions are subjective, the data indicate that there were no differences among models in the proportion of positively terminated participants who were placed in unsubsidized employment (61% of Model I; 65% of Model II; 63% of Model III).* (Interpretation of these data is difficult because a greater proportion of Model I participants were negatively terminated than of Models II and III. See Table 8.)

TABLE 8

TERMINATION STATUS BY MODEL

	Model		
Termination Status	I	II	III
Positive			
Non-Subsidized Employment	30%	40%	39%
Other	17%	20%	22%
Negative	52%	39%	39%
TOTAL N	126	109	134

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* Since Miami was the only site that provided the CRA with reliable data on post-program placement, analysis of these data is not presented in the section on New York or Albuquerque.

A problem for AYES participants was the depressed labor market. Throughout 1980 and 1981 unemployment was increasing, and cutbacks in government programs served to exacerbate the problem. In addition, AYES participants had the added liabilities of being young, unskilled, minority group members, many of whom had criminal records. The former E&T Director cited as an example the hiring preference of many beach hotels (a major source of employment in the Miami area) for Jamaicans, Cubans, and Haitians, who they felt were harder workers than the typical AYES participant (most of whom were native-born Blacks).

Manufacturing jobs were largely unavailable to AYES participants since most were located in North and West Miami, which are inaccessible from the inner city where most AYES participants lived. The lack of an adequate public transportation system in Miami compounded the problem.

The AYES population included large numbers of recent immigrants. The Haitian participants generally had language problems (most spoke only Creole), lacked work experience, and faced racial discrimination. Since Miami is a city where Spanish is spoken in many businesses, language was less of a barrier for the Cuban refugees. The recent immigrants from Cuba, however, had a bad reputation of having been criminals in Cuba, while the Haitians developed a reputation for being hard workers.

Racial discrimination was cited by the job developers as a major problem. One job developer felt that this was especially true in the department stores, where she thought the pre-employment tests were discriminatory and non-valid. Furthermore, as a result of the 1980 riots in Miami, some employers were reluctant to hire Blacks. The Project Director indicated that competition from the Haitians was a problem for American Blacks, although he considered it less of a problem than did some of the other staff members.

CHAPTER FOUR: ALBUQUERQUE

A. INTAKE

As in New York, the plan for intake was altered during the planning period from a policy calling for two distinct phases of intake to one stipulating a continuous intake procedure. Albuquerque intake, which began on August 11, 1980, was particularly slow and required over six months (until March 1981) before the first 225 participants could be taken into the program. The protracted length of intake necessitated a major revision of the Grant Plan, and random backfill was not implemented during this time. Since intake was scheduled to be completed before the end of March, there remained only three weeks to take in experimentals via random assignment to model; consequently, when intake ended on March 20, only 60 randomly assigned experimentals had been taken into the program. Although DOL had required approximately equal numbers of guided choice and randomly assigned experimentals, random assignments constituted only 20% of the Albuquerque sample.

Program operators in Albuquerque found it particularly difficult to attract CJ referrals to the program. In order to expedite the intake process, in October 1980, the DOL requirement of a minimum of 50% CJ referrals was lifted from Albuquerque AYES. In total, only 31% of the Albuquerque participants were CJ referrals. Table 9 presents a brief description of the Albuquerque experimental group.

TABLE 9 (n=295)

Referral source	
CJ	31.4%
non-CJ	68.6%
Assignment type	
guided choice	79.7%
random	20.3%
Sex	
male	59.5%
female	40.5%
Had diploma?	
yes	55.7%
no	44.3%
Ethnicity	
White	10.8%
Black	7.1%
Hispanic	78.7%
Native American	3.4%
Mean Age	18.8

Outreach

Prior to the inception of intake, the following agencies were contacted for potential candidates for AYES: federal, state, and county probation agencies; alternative schools; social service agencies; and community organizations. In addition, posters were displayed at community centers, and AYES staff spoke at various communities believed to be potential sources of AYES participants. When the initial turnout proved to be much slower than expected, a new series of outreach strategies were employed including: distributing leaflets in economically depressed neighborhoods; gaining more extensive media coverage; and extending liaisons with the Employment Securities Division, Youth Development Inc. (YDI), various drug programs, the Equal Opportunity Board, and Head Start.

Despite these efforts, Albuquerque program operators encountered major difficulties attracting the expected numbers of program candidates, particularly CJ referrals. In fact, this was apparently the largest jobs program that the City had ever implemented. Although AYES staff had attempted to improve their liaisons with probation agencies, two obstacles were never successfully overcome: the large caseloads of individual probation officers detracting from their ability to make referrals and to follow up on their clients; the hostility of the probation officers towards the experimental design, which assigned half of their referrals to the control group.

Finally, several members of the AYES staff reported that certain community groups stopped sending referrals to AYES because they perceived that their referrals were being discriminated against in the experimental/control assignment. Although Vera requested AYES staff to ask these agencies for a list of their referrals so that an analysis of their allegations could be conducted, no agency or community group ever provided this list. At any rate, this attitude indicated a basic distrust of the AYES project on the part of several groups that were expected to send referrals to the program.

Intake continued to be very slow throughout the Fall of 1981 despite the removal of the 50% CJ referral requirement and efforts at improving outreach. From November 1980 until February 1981, Albuquerque AYES took in an average of only 23 participants per month. In February a new Project Director was hired. He attempted to promote the program through the local media and by increasing ties to existing referral sources. Although this "media blitz" never proved successful in bringing in large numbers of CJ referrals into the program, over 60 experimentals were taken into the program during March. It was believed by one senior site staff member that had OCETA, the Albuquerque prime sponsor, been more helpful initially in pushing for more media coverage of the project, AYES would not have had so much difficulty attracting high-risk youth to the program.

The four main sources for the Albuquerque sample were, in rank order: walk-ins, YDI, The Employment Securities Division, and CJ-referrals (mostly from individual probation officers). Since over 80% of the Albuquerque sample were phase one, guided choice experimentals, a comparison of the characteristics of phase one versus phase two participants is unwarranted. Moreover, the opinions of interviewed program staff about changes in the participant population over time do not lead to any consistent conclusions.

Eligibility Screening and the Intake Process

Intake was scheduled to require three days to complete the following tasks for all assigned experimentals: day one - eligibility screening, research interviews, assignment to experimental or control group, and debriefing (for controls); day two - reading tests; day three - guided choice interviews. All program candidates had to document that they were 16-21 years of age, not in school, not working, and CETA-eligible. Intake was run four to five days per week.

Intake was originally handled by counselors and WSS. During the first few weeks of the project, program operators were reported by interviewed personnel to be in "complete panic" over the intake process. This confusion was attributed to lack of planning, and program staff claimed that there was insufficient time to meet and iron out problems encountered during the intake process. Due to their unfamiliarity with the AYES and OCETA forms, program staff required over an hour to assess the eligibility of a given candidate. Claiming that they had never been properly trained how to administer these forms, intake forms were sometimes returned to OCETA and had to be redone. Despite these problems, program staff expressed the belief that the correct standards for eligibility were followed throughout the course of the project; nevertheless, a "few" candidates were incorrectly approved as CETA-eligible and had to be removed from the program.* Many program candidates lacked the proper documentation for CETA-eligibility and were requested to return at another time with the required documentation. Sometimes these candidates could not or would not return again for intake. Unfortunately, there are no available data indicating how many candidates failed to complete intake.

Intake was reported to be a major problem for continued program operations. Counselors, in particular, complained that their intake responsibilities interfered with their programmatic functions -- developing rapport with their clients and finding suitable program placements for them. Nevertheless, two factors mitigated the disrupting influence of intake on program operations. First, there were fewer program candidates in Albuquerque than in the other two sites. Second, in November 1981, YDI was subcontracted to conduct eligibility screening for AYES. According to the Director of Albuquerque AYES, YDI's staff was "very expert, very thorough."

Candidates for the program were scheduled to arrive in the morning. The site researcher and a member of the program staff then conducted a brief orientation of the program and the research. After this orientation, candidates were sent for determination of eligibility. Those who were found eligible were administered the Vera interviews by site researchers and were requested to return later that afternoon for the ETS interview. According to the site researcher, "very few" candidates failed to return for the second set of interviews. The Vera instruments were administered individually; the ETS interviews were conducted orally to the entire group of candidates.

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* These individuals were subsequently removed from the Vera database.

After the ETS interviews, the site researcher determined by the use of a random number table the experimental and control status of the program candidates. The experimentals were then referred to the program staff, who scheduled an appointment for a guided choice interview. Controls were told that they were included in the research but not in the program; they were given a $10 stipend and an appointment at YDI, where they could see if other programs were available for them. Debriefing the controls proved to be a problem because often there was no program person available. According to the site researcher, not all controls received appointments at YDI, whose funding was severely cut in 1981; therefore, the earlier controls may have been given more resources than the later ones. Since controls sometimes had to wait for over one hour for debriefing, this may have generated antagonism towards the program. As a result, the success rate for exit and follow-up interviews may have been negatively affected.

Intake to Program Start

It generally required 3-4 weeks after intake (mean 31 days) before an experimental could actually begin program participation. The main cause for this delay was the need for all experimentals to undergo a physical examination. It often required weeks before an exam could be scheduled and the program staff receive word of the results; moreover, if an appointment was missed, the entire process was often delayed an additional week or two. As a result, program operators decided to allow experimentals to begin their program participation prior to receiving their physical examinations; however, since Model I participants were officially city employees, they were still required to pass their physicals before starting program participation. Therefore, it took longer for Model I's to begin program participation than the Model II's or III's. There are no available data indicating whether the Model I participants had a higher no-show rate than the other model participants, however.

During phase one of intake, experimentals had a guided choice interview with a program counselor to determine their preferred model choice(s). (This occurred on Day Two of intake along with the TABE testing.) As in the other sites, model preferences were then sent to the site researcher, who assigned experimentals to model each Friday. An attempt was made to offer the candidate his/her first choice (no one received worse than a second preference), while also assuring that the three models filled up evenly. (While in the planning phase of the program there had been some concern that one model might be widely preferred over the others, this did not turn out to be a problem. The great majority of participants received their first choice.) For research purposes, this practice assured all guided choice experimentals an equal chance of receiving their model preferences; programmatically, this system enabled the program operators to avoid the potential staffing problems generated by widely divergent model sizes. In addition, each Friday the site researcher monitored the proportion of criminal justice referrals in the experimental and control groups.

Several other tasks had to be accomplished before a participant could start the program. There was paperwork to be completed, Adkins and VITAS to be scheduled and administered, and placements to be located. According to both Albuquerque Project Directors, program operators decided they would rather delay program participation until a "quality placement" could be located than immediately enroll participants and then locate a program slot for them. Due to this policy, at least 50 Albuquerque experimentals waited at least 6 weeks after intake before actually starting their program participation.

The lagtime between intake and program start in Albuquerque was the highest of the three sites; nevertheless, the no-show rate did not exceed the rate in Miami, although it was higher than in New York. The no-show rate was actually higher for the guided choice experimentals (14%) than the random assignments (5%). This development is most likely the function of the decrease in lagtime over the course of the project -- 34 days for the former and 23 days for the latter.

B. FIELD WORK

Albuquerque AYES offered two types of Model I experiences: workcrew placements on sites supervised by AYES staffs, where participants were taught a variety of construction skills; and "single-site" placements at non-profit or government agencies, where participants were given work experience at a variety of CETA-type slots. Approximately 57% of the participants were placed in the former and about 46% in the latter. (The totals exceed 100% because some clients received both types of Model I experiences.)*

Placement into one of these model slots depended on a variety of factors. According to senior staff, the participant's work history, reading level, and VITAS assessment were the key determinants of placement into single-site or workcrew slots; the more skilled or experienced clients generally were encouraged to take single-site placements because they offered greater opportunity for job placement upon completion of AYES. Additional factors for determining placement included the desires of the participant and the availability of single-site placements. Finally, some "problem cases" were assigned to workcrews so that they could be more closely supervised by AYES staff. Although the Field Operations Coordinator assumed ultimate responsibility for assigning placements, most placements were made by program counselors.

Almost 85% of the workcrew members were males; 62% of the single-site placements, which were often clerical positions, were female. According to the second Project Director, most (Hispanic) women in the area have been socialized to assume that the hard outdoor labor of the workcrews was "men's work"; moreover, many members of the program staff shared this sexual stereotyping.

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* According to both site research and AYES program staff, approximately 7-10 Model II participants were placed on workcrews during their participation in AYES. Random assignment and the policy of strongly discouraging model changes were cited as the reasons for this divergence from the experimental design.

The field operations staff stated that the principal goal of both single-site and workcrew placements was to encourage the development of proper work habits such as punctuality, maintaining a presentable appearance, cooperation, and reliability. In addition, Model I experience was expected to offer clients the opportunity to learn various skills and develop a more extensive work history.

Most program staff believed that the workcrew placement was less preferable than the single-site placement due to the latter's potential for job opportunities and the "low status" of physical outdoor work in New Mexico. The advantages of working on the crews were the opportunity to learn basic construction skills and the rewards of being able to see the physical product of one's labor. This latter advantage was facilitated by the policy of undertaking construction rather than disassembly and clean-up tasks. Some of the projects undertaken by workcrews included: building a baseball field; renovating the facilities of such agencies as the New Mexico Youth Diagnostic Center; and installing sprinklers at the Albuquerque Skills Center. Model I participants assigned to work crews initially took a five week course -- the Laborer's Training Program -- where they were taught a variety of construction skills. After completing that course, they were sent to AYES-supervised worksites, where they applied those skills and learned new ones. The WSS, each of whom generally had a crew of 7-10 participants, directly supervised and trained the participants, although occasionally a journeyman employed at the worksites assisted in the process. The skills taught included: making adobes; learning how to operate a variety of tools; working with concrete; erecting scaffolds; framing; painting; and laying tiles. Since the Laborer's Training Program was open only to participants over the age of 18, younger clients received all their training at the worksite; such clients learned all the above skills with the exception of the use of power tools (which AYES did not possess). The quality of work was described by site staff as being generally good.

Based on interviews with different members of the field staff, the type of instruction and level of discipline at the worksites appeared to vary. Some WSS emphasized individual instructions, while others taught skills to the entire group and then worked individually with those clients who were having problems. Some WSS' described themselves as strict disciplinarians, while others reported that they felt a "tolerant" approach was more beneficial. Model I participants had the lowest rate of negative terminations and averaged the greatest number of program hours of the three models -- probably an indication of the more relaxed discipline system within that model. Random assignment was said to have had little effect on Model I, and program data on termination rates and participant hours confirm that assertion. Most negative terminations were due to excessive absenteeism. Morale on the worksites was said to be high; the relationships among clients and WSS were also reported to be good.

The two main problems for the WSS were getting supplies and the relationship between the WSS' and the counselors. According to both the WSS' and the Field Operations Coordinator, operations at the worksites were frequently delayed due to problems getting supplies. Much of this problem stemmed from red tape, since all requests for supplies had to go through OCETA, the prime sponsor. The problem between the counselors and WSS seemed to be the function of several factors: poor lines of communication between the two staffs; the feelings of the WSS that the counselors sometimes denigrated workcrew placements; and ethnic rivalries (initially most WSS were Hispanic and most counselors Black or Anglo). This problem was said to have been ameliorated somewhat by personnel changes made during the course of the project.

The single-site placements were considered the choice slots within Model I. In fact, the Field Operations Coordinator reported that he "rewarded" several conscientious workcrew members with transfers to single-site placements. These placements were judged by site staff to have several advantages over workcrew placements: the increased likelihood of a transition into a regular job upon completion of AYES*; the lower client-supervisor ratio; participating in "the main stream" of the work world; and the "status" of an administrative/clerical position over to the manual labor of the workcrew. The positive termination rates on workcrews and single-site placements were approximately the same, however.

Selection of agencies for single-site placements was largely based on the agencies' experience with high-risk youth and the possibility of participants eventually being employed at those agencies. Unfortunately, few placements at these agencies ever resulted in post-program jobs (see Job Development). The selected agencies included: the Mental Health Center, City Parks and Recreation, libraries, the city accounting office, hospitals, the Albuquerque Skills Center, and museums. The work done at those agencies included: filing, typing, bookkeeping, serving as recreational aides, maintenance, cataloguing, and accounting. Clients were supervised by personnel employed at those agencies.

Field Representives and senior staff stated that the work done at these placements was usually good. They reported few morale problems or problems between clients and agency employees. Most clients negatively terminated in these placements were dropped from the program due to excessive absenteeism. Random assignment was said to be no problem. The main problem cited by Field Representatives was poor communication with the counseling staff.

Relationships with community groups and members of the communities near AYES worksites were described as good. According to the WSS', there was some initial fear of the AYES clients in some neighborhoods, but these fears were soon dispelled by the behavior and performance of the workcrews. On the other hand, there was little awareness on the part of the community of AYES as a distinct entity. Sometimes members of the communities brought food or drink for the workcrews, but very few jobs were located for AYES clients in those communities. Field staff claimed that the low rate of job offers was a function of the depressed economy (especially in the construction industry) rather than any negative attitudes towards AYES or AYES participants.

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* The evidence gathered on post-program placements do not support this perception, however.

C. EDUCATION AND TRAINING

As planned, Albuquerque AYES was able to place their clients in a number of community-based educational and vocational training agencies. As in the other two sites, selection of these agencies was based essentially upon the compatibility of the programs offered at these agencies with the goals of the AYES project and their experience with high-risk youth.

Placements for educational and skills training were made at the Albuquerque Skills Center, Phase III, Technical-Vocational Institute (TVI), and the Opportunities Industrialization Center (OIC). Most participants in Model II were placed in educational slots -- 55% in remedial classes and 41% in GED slots. An additional 47% received vocational training. Since the AYES population was comprised primarily of Mexican-Americans indigenous to the state and Anglos, only 2% of the AYES participants took ESL classes. The total excedes 100% because a large number of participants were placed in remedial followed by GED placements, or GED placements followed by vocational training placements.

Placement into one of the types of E&T slots varied over time. Participants' reading scores, educational levels, and interests were the essential criteria for placements; however, according to the second Project Director, these criteria were employed more systematically during the latter part of the project's duration than during the first few months of operation. He attributed this to personnel changes and the efficiency resulting from the experience of several months of program operations.

The quality of educational placements was rated by the E&T staff as good. The range of placements was rated as "good" to "fair." A major problem with both educational and vocational training placements was that the start dates of their classes often did not coincide with a participant's AYES program start date. As a result, the participant's AYES start date was often delayed until several weeks past their intake. Surprisingly, this delay had little effect on the no-show rate. The E&T and Project Directors both felt that most participants made significant progress towards their diplomas as well as significantly improving their math and reading abilities. These statements are supported by program data indicating that almost 8% of the participants in E&T received academic credit and 16.5% received a GED. These two rates are the highest of the three sites, although the significantly higher reading scores of the Albuquerque participants may be related to this finding.* On the other hand, the Project Director felt that the GED program at OIC was of questionable quality, and some GED recipients did not really "earn" their GED's; his basis for that assertion was that many participants who obtained their GED from OIC failed to pass the entrance examination for vocational training programs at TVI.

The range of vocational training placements was rated as "good" to "adequate." The classes offered to AYES participants included: job preparation programs and a wide variety of training programs at TVI; clerical, auto parts, electronics assembly, and word processing at the Skills Center; and industrial arts slots at Phase III. The quality of the placements was rated as excellent. Unfortunately, most skills training classes were too sophisticated and advanced for most AYES participants. Many clients were therefore placed in TVI's job preparation classes. These classes were on a more basic level than the skills training classes offered at TVI or the other placement agencies. Many participants placed in job preparation were transferred to skills training classes after completing the course. Only 3% of the vocational training placements received an occupation certificate and/or completed their skills training classes while in AYES; many of these participants, however, continued their skills training after their graduation from AYES.

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* According to the STEP test administered at intake, the mean score in Albuquerque was 14.2, as opposed to 12.8 and 8.6 in Miami and New York, respectively.

The Albuquerque E&T process was probably aided by the fact that there were only 60 clients who were randomly assigned to model. While there was only a slight difference between random and guided choice participants' positive termination rate, the difficulty of making appropriate placements was probably alleviated by a lower percentage of randomly assigned participants. The Albuquerque program also benefited from a wider and more versatile range of outside educational and vocational training slots than the other sites.

D. MODEL III

For their E&T sequence, 30% of all Albuquerque Model III's were placed in GED classes, 31% in remedial classes, 3% in ESL, and 34% in vocational training classes. For their work experience component, 29% of the Model III's were placed on AYES workcrews and 52% in single-site placements. According to program and site research staff, all but a few early terminations received equal exposure to both model components; nevertheless, program data indicate that at least 19% of all Model III participants never were placed in a work experience slot. Unfortunately, program data do not indicate length of participation within any one model component.

Although, according to the Grant Plan, Model III experience was supposed to be scheduled in either split-day or alternating two week placements in E&T and work experience, program operators found it generally unfeasible to follow this plan. Most outside agencies utilized for either E&T or single-site work experience placements could not accomodate participants scheduled according to the brief sequences mandated by the Grant Plan. As a result, most Model III participants spent sequential two to three month placements within each model component. Given the policy of placing most Model III participants in their E&T sequence prior to their work experience placement, it is likely that this development resulted in the exclusion of a number of Model III's from the latter placement.

In the opinion of senior staff in Albuquerque, it was not possible to operate an effective Model III within the confines of the Grant Plan. They claimed that the stipulated two week maximum within a given model component was never a viable alternative, given the requirements of the outside agencies used for E&T and single-site placements; moreover, the limited public transportation in Albuquerque rendered split-day schedules extremely impractical for most AYES clients. Second, they stated that six months was too short a time for a combined work experience/educational model to be effective. They argued that participants should have been given opportunity to improve basic literacy and math skills prior to work experience, but many participants had not made sufficient progress in their education for their work experience to be effective. Moreover, due to the unavailability of slots, they were unable to place all Model III's in their E&T sequence first; as a result, these clients did not receive an optimal program experience. According to program records, only 3% of all Model III's received any academic credit, 9.1% a GED, and 1% an occupational certificate while attending AYES. Finally, although the two components of Model III should have been related to each other, this was not always possible; consequently, Model III experience sometimes had "no continuity."

E. COUNSELING AND TERMINATIONS

In order to promote clients' employability, or "job readiness," the counselors had a two-fold role. First, they determined within the range of available model slots the best possible placement for a particular client; of course, random assignment restricted these efforts. Second, counselors tried to teach their clients proper work-related behavior (e.g., how to dress for a job interview, how to deal with conflict on the job, what employers expect from their workers, etc.).

The counseling component of the AYES program in Albuquerque was compromised, according to staff interviews, for a variety of reasons. First, the counselors had numerous roles, some of which reportedly were not clearly defined. For instance, counselors were delegated considerable responsibility during intake, and they felt that these responsibilities (including enormous amounts of paperwork) infringed on their primary responsibilities. Second, they also felt hampered by the size of their case loads and a lack of space in which to conduct personal counseling sessions. Finally, according to site personnel and the impressions of Vera Program Officers, there apparently was a lack of direction from site management during the first few months of operations; this problem was resolved by personnel changes made during the project's second phase.

As was the case in the New York site, there was some controversy over the type of counseling required for this project. The consensus among the site staff members was that the counseling should be geared toward helping the clients achieve their vocational goals, and helping them learn proper work-related behavior; many of these staff members felt, however, that personal counseling must be a necessary concomitant of vocational counseling. Counseling goals should also include, in part, gaining the confidence of the clients, values clarification, and personal problem solving. Despite this perception of the ideal counseling package that each client should receive, the Project Director felt that there were too many time and personnel constraints for intense personal counseling services. The E&T Director felt that in the later phases of an individual's participation in the program, more intensive counseling was conducted in order to determine an Employability Development Plan (EDP). Despite this supposed shift, it appears from staff interviews that the EDP's were never consistently developed or followed in the Albuquerque AYES program.

VITAS and Adkins were both significantly abridged. VITAS was perceived by the staff as marginally useful as an assessment tool but unwieldy to administer due to its length. The interviewed staff members alleged that some clients in the early phases of the program never received VITAS. This situation was rectified, however, and apparently in the later phases of the program all clients received VITAS.

Adkins was perceived as a "scheduling nightmare" and was eventually modified to smaller and fewer sessions. The job holding, resume writing and self-introspection parts of Adkins were perceived to be the most useful. In Albuquerque an additional problem arose in the administration of Adkins. Some outside training institutions (particularly TVI) did not excuse absences for Adkins; presumably, some clients placed at these agencies never received Adkins.

Interviewed program staff addressed the rift between the counseling staff and the work site supervisors. This strained relationship seemed to arise due to a lack of communication, despite informal meetings arranged with the goal of facilitating communication. Eventually, the Vera Program Officers requested formal meetings between the counseling and work site staffs, and these meetings alleviated the problem to a certain extent.

The E&T Director felt that she, the counselors, and the Project Director worked well together in deciding terminations. Absenteeism was the predominant reason for terminations; very few terminations were made for behavioral violations or attitude problems. Early in the program, there were some problems obtaining information about attendance/performance from these institutions, but this situation was rectified. Sometimes termination proceedings originated from an outside training or educational institution for reasons of absenteeism or poor performance. The institution's own policy determined a client's, but sometimesthe program kept the clients and transferred them to another site. More frequently, the termination proceedings were initiated by the program staff. Although the relationship between the counselors and worksite supervisors was strained, problem cases were referred from the sites to the counselors. In extreme cases, a referral was made to a psychologist. An attempt was made to resolve clashes between the clients and the WSS by switching the client to another site. Subsequent to such attempts, termination decisions were made.

If a participant had excessive absences, the termination could be made without the Project Director's approval. If the termination was made for another reason, the termination was recommended by the counselor to the E&T or Field Operations Director. The Project Director felt that attendance policy was "cut and dried" and that 80-90% of the negative terminations were made for excessive absences.

Table 4 presents the positive termination rates for random assignment vs. guided choice participants in each of the three models. There are no significant differences on the positive termination rate by model or for the entire Albuquerque sample. The impact of assignment type may be moderated by the fact that randomly assigned participants entered the program during a later and perhaps more efficient phase of operation.

TABLE 10

PERCENT POSITIVE TERMINATIONS

	MODEL			
	I	II	III	TOTAL
Guided Choice	75%	64%	61%	60%
Random	70%	63%	67%	67%

F. JOB DEVELOPMENT

As in Miami and New York, the goal of job development in Albuquerque was to place AYES participants in jobs or skill training programs that were related to their "immediate employment goals." For a variety of reasons, adherence to this goal proved all but insurmountable for the Albuquerque AYES project.

At the start of the program, faced with last minute budget reductions, Albuquerque program operators decided that their most pressing personnel priority was to hire vocational counselors rather than job developers; consequently, they opted to subcontract job development and placement services at Youth Development, Inc. (YDI). This organization reportedly had an excellent record for placing high-risk youth in unsubsidized jobs; moreover, by centralizing the job development process they would be able to control the flow of job applicants to potential employers, and they could direct the most job-ready program participants to job interviews. In addition, YDI offered to make its job preparation course available to AYES participants. This course included seminars on how to act at a job interview, how to complete a job application, and how to write a resume.

Unfortunately, the ability of YDI to place AYES clients in jobs was extremely disappointing due to YDI's own funding cuts and staff turnover. Moreover, AYES' funding limitations, in conjunction with unfounded hopes for an improvement of YDI's placement record, led program operators to eschew organizing an in-house job development unit until the program had almost concluded.

Several other problems were reported by program staff as limiting the success of AYES' job placement services. Internally, there was a great deal of turnover within the AYES E&T unit. With this unit not operating under peak efficiency, there was often no one responsible for monitoring YDI's services or organizing and supervising AYES' in-house job development. Externally, the poor state of the

Albuquerque economy (especially the high unemployment within the construction industry and the city's freeze on hiring) limited the number of positions available for AYES graduates. Finally, many potential employers chose not to hire AYES graduates, who were usually young minority-group members with limited employment histories, low educational levels, and often with criminal records.

In the face of this adversity, AYES reorganized its in-house job development services in June, 1981. According to the Project Director, program operators concentrated on what he called "self-directed" job placements. That is, although program staff continued to promote job-readiness of program participants and attempted to find jobs through formal and informal networks, it was expected that AYES participants essentially would have to locate their own employment after leaving the program.

Under the new system, WSS and vocational counselors were asked to act as informal job developers, and supervision of AYES job placement efforts was placed under a former Field Representative. Program operators also organized a Job Fair for AYES participants.

Nevertheless, there continued to be several limitations on the effectiveness of the in-house job development services:

1. there were no stated performance goals for the job development unit;
2. there was no policy for conducting follow-ups on clients placed in jobs;
3. there was no standard definition of job-readiness, nor was there any systematic method employed for evaluating the job-readiness of terminating clients;
4. there was a general failure to prepare comprehensive EDP's for AYES clients;
5. the poor communication between field staff and counselors precluded more effective cooperation between these two units;

6. according to the E&T Coordinator, while Adkins sessions and YDI's job preparation classes were useful for some AYES clients, they were a "waste of time" for those clients with more advanced work skills.

Reflecting the lack of coherency in the job development services at AYES, estimates of the percentage of graduating participants who were job-ready varied from 20-60%. Most program staff agreed that participants placed in single-site Model I slots or Model II vocational training slots were the most job-ready AYES participants. This pattern was attributed more to the policy of placing the more skilled participants in these slots than to the greater effectiveness of these program slots. It was further noted that TVI, an agency where most vocational training placements were made, had an overall 80-90% job placement record for its graduating students. Unfortunately, most AYES clients placed at TVI were unable to graduate while attending AYES; most of these clients continued their vocational training at TVI after graduating from AYES.

Data provided by program staff about post-program job placements are inconsistent, thereby making it difficult to evaluate the job development services at Albuquerque AYES. According to data on the IPP, 35 participants were either employed (32) or under-employed (3) when they left AYES; but this figure is contradicted by another IPP item indicating that 37 participants had been placed in unsubsidized jobs after leaving AYES. Since the latter figure should be lower than the former, the discrepancy between the two is apparently a function of site staff's inability to provide Vera with more accurate data. Finally, according to a document distributed by program operators, a total of 86 participants were listed as AYES job placements. Although it is impossible to determine accurately the validity of these data, it appears likely that the lower estimates reflect the number of placements made immediately subsequent to participant's termination from AYES, while the higher figure includes a large number of "self-directed" placements located some time after leaving AYES. According to this list of 86 placements, all but a handful were earning less than $4/hour. Most of these jobs were "entry-level positions," usually in fast-food franchises or in the service sector (e.g., maintenance, child-care, and domestic jobs). According to program staff, most clients appeared to be satisfied with their employment (although, as mentioned before, there was no system for making follow-ups on graduated clients).

Appendix B

Methodological Notes

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Appendix B - Methodological Notes

Analysis of Sample Attrition

As is described in Chapter II, the AYES research was a longitudinal study of program impact on employment and crime variables. Researchers in the three project sites made every effort to maintain contact with the 2220 research subjects through the eight-month follow-up period. Nonetheless, as is inevitable with this type of research, there was some sample loss. As is presented in Table II (p. 28), site researchers were successful in interviewing 69 percent of the experimental group and 58 percent of the control group at the time of the 8-month follow-up. Given this attrition, it was necessary to do some data analysis to determine whether the remaining experimental and control groups were comparable at program entry.

The "attrition analysis" focused on the demographic variables discussed in Chapter III and on selected employment and crime variables describing research subjects' experiences prior to intake. Two independent variables -- site and treatment -- were used in each analysis; thus, we were able to compare the experimentals and controls in each site. Only those research subjects (N=1383) who had received a Vera eight-month follow-up interview were included in the analyses.

The results of the analyses on the demographic characteristics of this subsample of AYES subjects are presented in Table B-1. For comparative purposes, this table has the same format as Table 3 (p.33). The analyses on referral source, gender, having a diploma at intake, and ethnicity were computed using crosstabulations and chi squares. A 3 X 2 analysis of variance with site and treatment as the independent variables was computed on age at intake. The analyses on demographics produced significant treatment effects on referral source in Albuquerque and Miami, and on gender in New York.

TABLE B-1

Demographic Characteristics Of The AYES Research Subjects Interviewed at 8 months after Exit

	ALBUQUERQUE			MIAMI			NEW YORK			ALL SITES		
	E n=220	C n=190	TOTAL n=410	E n=286	C n=250	TOTAL n=536	E n=230	C n=207	TOTAL n=437	E n=736	C n=647	TOTAL n=1383
Referral Source												
Non-CJ (%)	67.7	76.3	71.7	54.5	45.2	50.2	46.5	44.9	45.8	56.0	54.3	55.2
CJ (%)	32.3	23.7	28.3	45.5	54.8	49.8	53.5	55.1	54.2	44.0	45.7	44.8
Age at intake (mean)	18.8	18.8	18.8	18.7	18.5	18.6	19.0	19.0	19.0	18.8	18.8	18.8
Gender												
Male (%)	59.1	54.2	56.8	66.1	63.1	64.7	65.2	74.4	69.6	63.7	64.0	63.8
Female (%)	40.9	45.8	43.2	33.9	36.9	35.3	34.8	25.6	30.4	36.3	36.0	36.2
Had a Diploma at Intake												
Yes (%)	45.9	47.3	46.5	21.3	16.7	19.2	9.2	10.1	9.6	24.7	23.0	23.9
No (%)	54.1	52.7	53.5	78.7	83.3	80.8	90.8	89.9	90.4	75.3	77.0	76.1
Ethnicity												
White (%)	10.0	10.0	10.0	3.2	0	1.7	2.6	0.5	1.6	5.0	3.1	4.1
Black (%)	4.6	4.2	4.4	80.4	83.2	81.7	68.1	84.5	75.9	53.8	60.4	56.9
Hispanic (%)	83.2	84.7	83.9	16.4	16.8	16.6	29.3	15.0	22.5	40.4	36.2	38.4
Indian (%)	2.2	1.1	1.7	0	0	0	0	0	0	0.7	0.3	0.5
Asian (%)	0	0	0	0	0	0	0	0	0	0	0	0

In Albuquerque there was a significantly[1] higher proportion of criminal justice referrals in the experimental group (32.3%) than there was in the control group (23.7%). While this is a statistically significant effect, it represents a weak relationship (phi = -.095). Furthermore, inspection of Table 3 reveals that there was a similar disparity between the experimental and control groups as a whole. Because the effect is quite weak, and there were no other significant differences between Albuquerque experimentals and controls, it is safe to consider the two Albuquerque groups comparable despite sample attrition.

The Miami sample also evinced a significant relationship[2] between referral source and treatment. In Miami, however, there was a significantly greater proportion of criminal justice referrals in the control group (54.8%) than there was in the experimental group (45.5%). This too was a weak relationship (phi = .093) and similar to the relationship in the Miami sample as a whole. Thus, it appears that the Miami research sample retained its comparability.[3]

The analyses of the New York data revealed significant relationships of treatment with both gender[4] and ethnicity.[5] As was the case in the New York sample as a whole, both Hispanics and females were over-represented in the experimental group. Neither of these effects was strong enough to have an impact on the outcomes of the research. Gender was used as a predictor or covariate in the analyses of outcomes, and in all cases entered the analysis before the treatment variable. As was discussed in various places in the report, we were unable to use ethnicity as a predictor variable because of its distribution in the three sites.

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[1] $X^2 = 3.707$; df=1; $p = .05$

[2] $X^2 = 4.66$; df=1; $p = .03$

[3] While the analysis of ethnicity in Miami produced a significant relationship with treatment ($X^2 = 8.00$; df=2; $p = .02$), inspection of Table B-1 reveals that this is due to the distribution of white subjects (n=9), all in the experimental group. If the chi square is recalculated for blacks and hispanics, it becomes clear that there is no relationship between ethnicity and treatment ($X^2 = .003$; df=1; n.s).

[4] $X^2 = 4.34$; df=1; $p = .04$; phi= -.100

[5] $X^2 = 13.67$; df=1; $p < .001$; phi=.179

Thus, while the analyses on demographic variables produced some statistically significant effects, none of these was strong enough to affect the outcome of the research. In general, the demographic characteristics of the 1383 research subjects who received an Vera eight-month follow-up interview were very similar to those of the AYES sample as a whole. In addition, in each instance of a significant difference between experimentals and controls in the reduced sample, that difference was reflected in the original sample as well.

Having concluded that the reduced sample of experimentals had equivalent demographic characteristics to the reduced sample of controls, we did additional tests on their employment and arrest experiences prior to AYES intake. The employment variables in these analyses were weekly earnings on the most recent job in the year prior to AYES intake and the percent of time worked during that year. In each analysis, all 1383 subjects who had received an eight-month follow-up were included. Thus, any respondents who did not work at all during the year prior to intake received a value of zero on the two employment variables. A 3 X 2 analysis of variance was computed on each of these variables, with site and treatment as the independent variables. Treatment did not have a significant effect in either analysis, while there were significant site effects on both employment variables. The means for weekly earnings are presented in Table B-2 and the means for percent of time worked are in Table B-3. It is clear from Table B-2 that there were no differences between the earnings of experimentals and those of controls in this subsample.

Table B-2

Mean Weekly Earnings Prior to Intake for AYES Subjects Interviewed at 8 Months after Exit

	Site*			
	Albuquerque	Miami	New York	TOTAL
Experimental	$71.64	$67.39	$58.78	$65.97
(N)	(220)	(286)	(230)	(736)
Control	$70.11	$73.36	$58.31	$67.59
(N)	(190)	(250)	(207)	(647)
Total	$70.93	$70.17	$58.56	$66.73
(N)	(410)	(536)	(437)	(1383)

* $F(2, 1377) = 6.00$; $p = .0025$

Table B-3

Mean Percent of Pre-Intake Year Employed for AYES Subjects Interviewed at 8 Months after Exit

	Site*			
	Albuquerque	Miami	New York	TOTAL
Experimental	22.9	17.6	16.7	18.9
(N)	(220)	(286)	(230)	(736)
Control	22.3	21.7	13.2	19.1
(N)	(190)	(250)	(207)	(647)
Total	22.6	19.5	15.0	19.0
(N)	(410)	(536)	(437)	(1383)

* $F(2, 1377) = 9.51$; $p = .0001$

Furthermore, the mean weekly earnings for the sample as a whole was $67 (p.54), which is virtually identical to the mean for the reduced sample. The results on percent of time working are similar. There is no significant difference between experimentals and controls; nor is there a significant interaction effect. For the sample as a whole, the mean percent of the year prior to intake employed was 18.4 (Table 13, p.52), and the mean percent of the year employed for the subsample was 19.0. Thus, it appears that the 1383 subjects (62% of the total sample) who remained in the research through the eight-month follow-up not only remained equivalent on employment variables measured at intake, but are also representative of the 2220 subjects who entered the AYES research.

The final dependent variable in the attrition analyses was the number of arrests during the two years prior to intake. Again, only subjects who were interviewed eight months after exit were included in this analysis. The 3 X 2 analysis of variance produced a significant effect for site[6] and a significant site X treatment interaction.[7] (The effect for treatment was not significant.) The means are presented in Table B-4. While there was no significant difference

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Table B-4

Mean Number of Pre-Intake Arrests for AYES Subjects Interviewed at 8 Months After Exit

	Site			
	Albuquerque	Miami	New York	TOTAL
Experimental	.74	.98	.57	.78
(N)	(214)	(283)	(225)	(722)
Control	.51	1.05	.87	.83
(N)	(189)	(243)	(201)	(633)
Total	.64	1.01	.71	.80
(N)	(403)	(526)	(426)	(1355)

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[6] $F(2,1349) = 8.12$; $p = .0003$

[7] $F(2,1349) = 3.11$; $p = .04$

between the mean number of arrests for experimentals (.78) and that for controls (.83), the means for the two groups within Albuquerque and New York do appear to be different. In each case, however, the direction of the difference parallels that for the sample as a whole. And, while these initial differences between the experimental and control groups are larger than any of those discussed above, the post-intake arrest variables are not affected by sample loss because these data were collected from official records for all subjects rather than from personal interviews. For ease of comparison the mean numbers of pre-intake arrests for the entire sample are presented in Table B-5. Clearly the arrest rates for the reduced sample are representative of those for the entire AYES sample.

Table B-5

Mean Number of Pre-Intake Arrests (Complete Sample)

	Site			
	Albuquerque	Miami	New York	TOTAL
Experimental	.71	.94	.68	.80
(N)	(289)	(373)	(399)	(1061)
Control	.57	.90	.81	.77
(N)	(320)	(383)	(408)	(1111)
Total	.64	.92	.75	.79
(N)	(609)	(756)	(807)	(2172)

Taken together, the analyses on demographic characteristics, pre-intake employment experiences, and pre-intake arrests for the reduced sample provide convincing evidence that the remaining experimental and control groups were comparable at intake. In addition, comparison of these data with intake data for the complete sample imply that the reduced sample closely approximates the original sample on every variable studied. For each instance of a significant experimental/control difference, a similar effect was evident for the sample as a whole.

VDOL(AA1) Data Processing Report / Results, Development 12/31/82

Vera Institute of Justice
Alternative Youth Employment Strategies (AYES)
December 31, 1982
J. D. Best, Senior Data Processing Analyst

The above-captioned project, also known internally as the Vera Dept. of Labor(VDOL) project, was completed on this date; below is a summary description of the data collection and processing procedures. This large database could not have been processed without the assistance of Ronald R. Erickson, who implemented the data logging control system, and June Swerdlin, in the technical operation of the control and dataset-update systems.

(I) Results of processing.

(A) An IBM-labeled tape (1600 BPI) containing a copy of the AYES master input dataset, described in section V below.

(B) A printed listing of the AYES processing program library, and an analysis, by case, of the contents of the master input dataset.

(C) A number of chronological volumes of printed analysis reports; these are cross-referenced by date in the program library index to the program which created them.

(D) An IBM-labeled tape (1600 BPI) containing a copy of the AYES master transmission dataset, specially formatted for installation in a national database, as specified and maintained by Educational Testing Services, Princeton, NJ.

(II) Development hardware and software.

The research database was developed on an IBM computer system (City University of New York) from multiple files of data keypunched onto 80-character records. It was prepared and analyzed with a number of statistical software systems; file description and analytic procedure syntaxes are described in the following sources:

DTXT Datatext (Cambridge Computer Associates 3.1X)
Armor and Couch, Editors (1972)
Collier Macmillan / Free Press
866 Third Avenue
New York, NY 10022

SAS Statistical Analysis System, 1979
P.O. Box 10066
Raleigh, NC 27511

SPSS Statistical Package for the Social Sciences
McGraw-Hill Book Co. (1970, 2nd Ed.)
Princeton Rd., Hightstown, NJ 08520 (609)448-1720

(III) Database description.

The analyses were performed on a research sample of 2220 subjects, consisting of youths eligible for participation in the AYES program, in three US cities. Data were collected from four subject interviews, criminal and juvenile justice system records, and AYES program records. Collection took place during the period July, 1980 through May, 1982; this information was coded and entered onto disk data files continuously during that period.

These data have been embodied in one dataset of up to 47 (80-character) records per unit. The dataset layout is described below. Variable layout is presented in the processing program library, arranged alphabetically by subfile mnemonic.

(IV) Error screening and updating.

Prior to analyses, all variables were tested for out-of-range values and other logical inconsistencies; offending units were flagged and omitted from analyses involving these variables. Extensive updating of datasets was performed, both to correct improper data entry and reflect collection of more recent information. Analyses were then rerun on the updated file.

(V) Input Dataset Layout

The input dataset consists of six waves of data, plus two records containing computed test Scores, for a total of up to eight subfiles per case. The 47 records are identified as follows:

COLUMN	CONTENTS
1-4	Sequential case identifier
5	Check digit (base-10)
6-7	Card number (01-47)
8-80	Data fields, specified in the program library

Subfile Mnem/Cd-No	Total Cases	I/O CHARACTER FORMAT			INTERNAL SYSTEM FORMAT		
		RECS	TOT RECS	TRACKS	N VARS	D/PTS (K)	TRACKS
Data Log NA	2220	1	2220	10	8	17.76	
INT 1-11	2224	11	24464	106	598	1329.95	338
INTS 12	2224	1	2224	10	18	40.03	
XIT 13-24	1702	12	20424	89	567	965.03	258
FU1 25-33	1152	9	10368	45	424	488.45	124
XITS 34	1702	1	1702	8	17	28.93	
FU2 35-43	1384	9	12456	54	447	618.65	157
CJS 44-46	2208	3	6624	29	114	251.71	65
XUP 47	34	1	34	1	33	1.12	1
Raw input totals:		48	80516	352	2226	3741.64	943
Master DB	2220				2156	4786.32	1154
Analysis DB	2220				708	1571.76	475
Transmit DB	2220 X	2171	=4819.62	K-BYTES	723	1605.06	278